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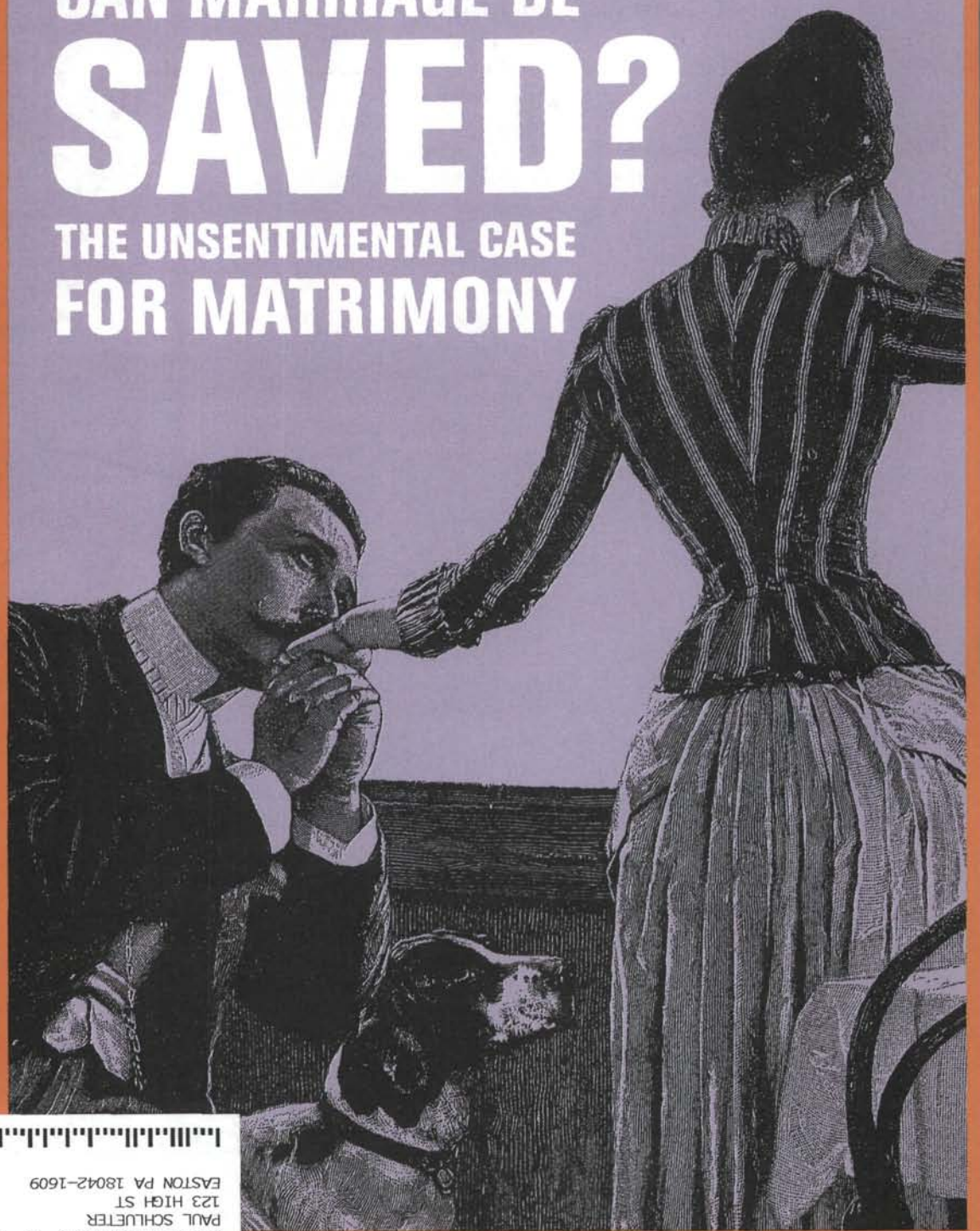

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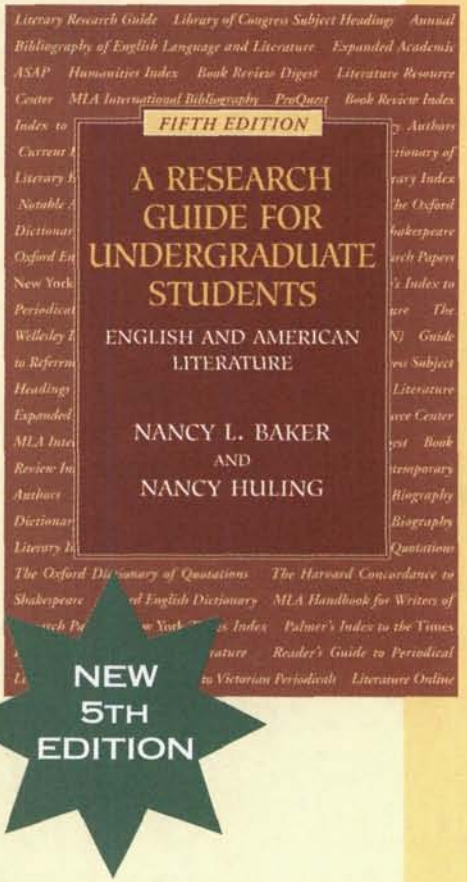

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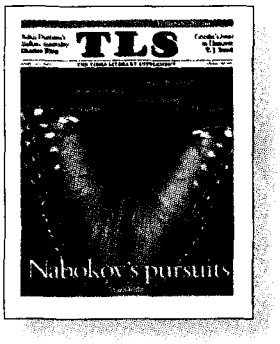

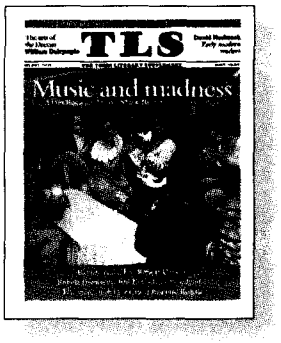

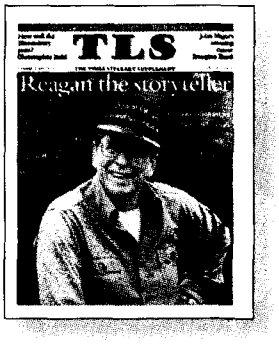

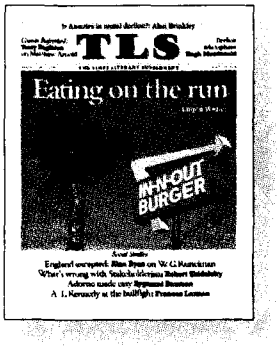

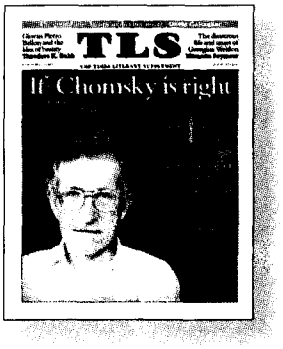

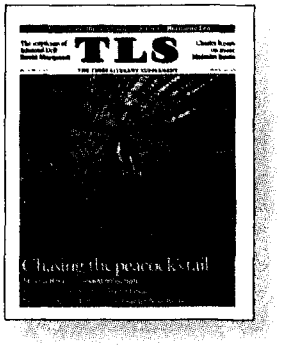

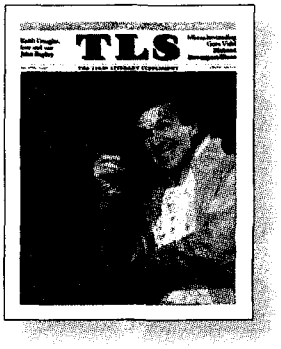

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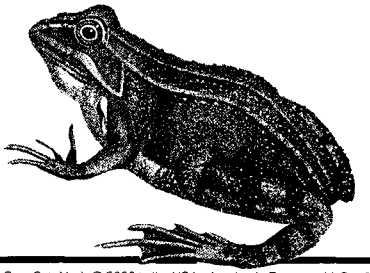

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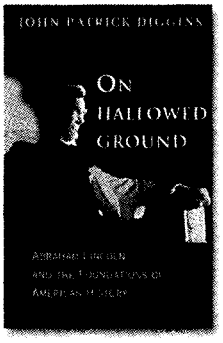

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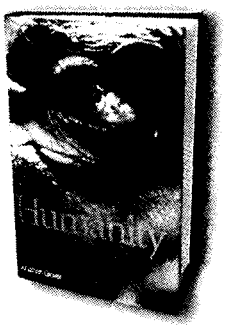

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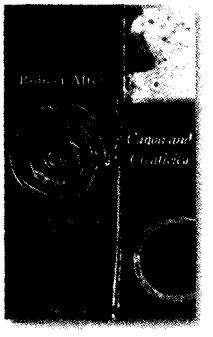

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RADICAL POETRY

As one who was radicalized during the Vietnam War, I was puzzled by Andrew Epstein's comment ["Verse vs. Verse," September] that most of the Language poets were "radicalized by the Vietnam War and severely disillusioned by Watergate." If true, they couldn't have been very radicalized. Perhaps Epstein meant to write "liberalized," which would also help to explain why some of these poets are serving capitalism as advertising and market analysts.

Far from being disillusioned, people like me found Watergate a delightful confirmation of our analysis of American society.

JIM BLYTHE
Professor of History
University of Memphis

The original logo for L=A=N=G=U=A=G=E, mentioned in "Verse vs. Verse," was designed by the poet Ray DiPalma. Language poetry was influenced not only by French thinkers but also by contemporary French poets, who, in a violent reaction both to surrealism and to Resistance poetry, centered their poetics on redefining *both* the signified and the signifier (to play off the then jargon!). Their impact was doubly felt in the United States: first through translations and second through the regular visits of French poets of the 1960s such as Francis Ponge, Edmond Jabès, Emmanuel Hocquard, Claude Royet-Journoud, Anne Portugal, and Joseph Guglielmi. American poets have in turn visited Royaumont (outside Paris) for collective translation exercises. Thus the L=A=N=G=U=A=G=E poets in their revolt against bourgeois poetics found a perfect French parallel reaction not only with Derrida but also with their French counterparts.

SERGE GAVRONSKY
Professor of French
Barnard College

CAMPION CRISIS

I was amused to read Hillary Frey's "Purloined Piano?" [Field Notes, September], because Jane Campion's unacknowledged use of Jane Mander's novel has been known for some time now. In "Silence, Sex, and Feminism: An Examination of *The Piano*'s Unacknowledged Sources" (*Literature/Film Quarterly* 26 [1998]), I explored the obvious parallels between the two works, as well as the out-of-court settlement between Mander's estate and Campion. I had real difficulties publishing the article in the most prestigious American film journals; none of them wanted to touch the subject.

Campion is a skilled adapter of the works of others (witness her version of *Portrait of a Lady*), and her early work, such as *Sweetie*, is strong because it is imbued with the director's own life experiences. When Campion adapted *The Piano*, however, she was forced to conceal her source—hence the heroine's muteness. I do not condemn her use of the Mander novel, but I do chastise her for silencing and appropriating the work of another New Zealand woman artist.

DIANE LONG HOEVELER
Professor of English
Marquette University

MORE TESTIMONY

In discussing *Shostakovich Reconsidered*, the book I co-edited with Allan Ho, Paul Mitchinson ["The Shostakovich Variations," May/June] delivers what he thinks is a coup de grâce to our "smoking gun" piece of evidence, the statement of Flora Litvinova. In the statement, Litvinova recounts meeting Shostakovich, who told her of his meetings with Solomon Volkov, "a young Leningrad musicologist" to whom he tells "everything" he remembers about his life and works. This seems a firsthand corroboration of the genesis of *Testimony*, which was related to and edited by Volkov.

"Not so," says Mitchinson. "Litvinova was referring to another corroborative work—a preface Volkov wrote to a book about Leningrad composers, and, besides, she met with Shostakovich much earlier, in 1971, before the work on *Testimony* had begun."

I find it very amusing that the *Lingua Franca* editors, who called us to ask if Allan Ho's university is called Southern Illinois University–Edwardsville or Southern Illinois University *at* Edwardsville, did not bother to verify this assertion. Neither, apparently, did Mitchinson.

continued on page 64

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NO TENURE, NO PEACE

REJECTION HURTS—AND FEW rebuffs sting more than the unsuccessful tenure bid. In the old days, you might have worked through that pain with your therapist or your loved ones. Now the Web offers a new option: Howl away into the void. Your friends and colleagues can then download your grievances and judge just how badly you were wronged.

John McLaren chose this tack after Columbia's economics department passed on promoting him last year. Not that McLaren, who studies trade theory and the regulation of commodity markets, was left out in the cold: The University of Virginia, Boston College, and the University of Wisconsin stepped in with tenure offers. And yet in August, after he settled on Virginia, he sent a change-of-address e-mail to 180 of his closest friends, directing them to a five-thousand-word rant titled "The Worst Mistake I Ever Made" (www.geocities.com/jem18_99).

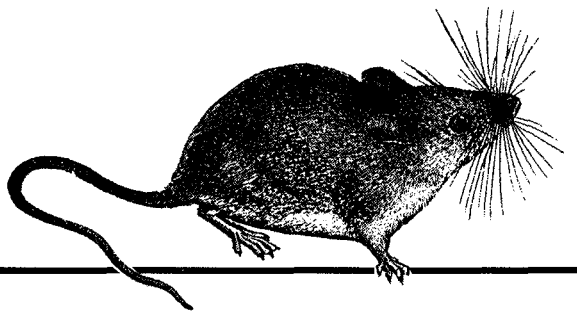
The mistake, apparently, was accepting a tenure-track job at Columbia after earning his Ph.D. from Princeton. "I did not know then what I know now about the pathologies, the cynicism, and the lack of principle that rule at the economics department at Columbia," writes McLaren, adumbrating problems that range from deadwood professors to departmental carpeting marred by "large and persistent stains."

At the core of McLaren's diatribe is a significant complaint: that Columbia's economics department views junior professors as underlings "to be flattered and fleeced." His own career in the department is a case in point. Recognizing him as a "nice guy" and an "easy mark," the department's chairmen deluged him with administrative chores, McLaren contends. Early on, McLaren served on a committee charged with hiring new assistant professors. The committee, he writes, was stacked with junior people, even though its task "was so overwhelming in its scale and so open-ended" that it left very little time for writing and research.

In his seven years at Columbia, McLaren was placed on the committee three more times, although there is a "sort of consensus in the profession" that no "ethically run department" would burden a junior professor so. What's more, for most of McLaren's time at Columbia, he taught two courses a semester—one of which was almost invariably a course he had never prepared or taught before. He also found himself taking up the slack for senior professors who served as principal advisers for Ph.D. candidates, "apparently without ever reading the dissertation."

Though he strained under the weight of this albatross, McLaren graciously endured: "I honestly had to exert a gigantic effort to get any research done at all. Nonetheless, somehow, through nights and weekends and a quantity of teeth gritting and elbow grease, I did. I'm pleased to report (at the same time as I blush to mention) that I scored a number of high-profile successes in leading journals.... Somehow, I learned to dance with those anchors chained to my ankles."

McLaren expected to be rewarded. Instead, he laments, he suffered a fate he describes as something out of *Goodfellas*: He was rubbed out. In the year he came up for review, Columbia hired five tenured professors, including two trade economists, Donald Davis and David Weinstein. McLaren says he was assured that these hires—in his field—would not hurt his chances. But he alleges that when his tenure bid was rejected, department chairman



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Richard Clarida told him an imbalance of trade economists was the reason.

As if all that weren't enough, writes McLaren in a postscript to his essay, Columbia's International Affairs Building, which houses the economics department, was "possibly the ugliest and dreariest structure in the hemisphere": The elevators were slow, the ceiling in his office once partly collapsed, his bookshelves sagged, and mice had the run of the place. Not just mice, either. He switched on the light in a men's room late one evening only to find every surface teeming with cockroaches. "The memory," he writes, "still grips me occasionally."

Then there were those "ancient and drab carpets."

McLaren assails Columbia's economics department for misuse of junior faculty and for roach-infested bathrooms.

Since its posting in August, the rant has attracted more than two thousand hits and has set McLaren's friends and former co-workers abuzz. Reached in Charlottesville, McLaren stresses his upbeat attitude. "The one thing that I don't want is for people to think that this is a bitter, angry, little man who put this up," he says. "In fact, I'm a really happy guy." He points out that because his wife, a psychologist, also got a tenure-track job at UVA, it's entirely possible he would have ended up there in any case. Does he harbor any bitterness? Perish the thought. Last year is "water under the bridge as far as I'm concerned." As for the Web site, he says he hoped it would inspire internal reform. "I like to think of it as a very positive document," he adds.

That's not quite the way it was received at Columbia. Clarida, chairman for the past three years, discussed the posting with junior faculty. "We are taking no specific steps to change anything, because nothing needs to be changed," he says. "We view John McLaren as a success story. He was very productive here—so productive that he had three tenure offers." Moreover, Clarida says he worked out a reduction in McLaren's teaching load, and that of other professors, from four courses per year to three in 1998. Administrative burdens, he maintains, are distributed as evenly as possible.

Was McLaren unjustly edged out by other hires in his subfield? Considerations of departmental balance do make a difference for junior professors, notes Jagdish Bhagwati, the most prominent trade economist at Columbia and McLaren's mentor. If the department has no one at all in your field, your chances of promotion are slim. Similarly, it may be difficult to build a coalition in your favor if there's a perceived overload of faculty who share your specialty. However, in Columbia's case, Bhagwati says, there is no such overload: In fact, the department has an offer out to Princeton's Giovanni Maggi, yet another top international economics specialist. As for the overwork question, Bhagwati says: "Since we want to hold on to good people, the tendency is to bend over backward to accommodate young people. I was a bit startled to see John writing in the way he did. If he had a problem, he should have come to me."

All well and good. But what's the deal with that building? "I would love to have the building renovated, as would anyone who has ever taught here," Clarida says. "That's not a unique view."

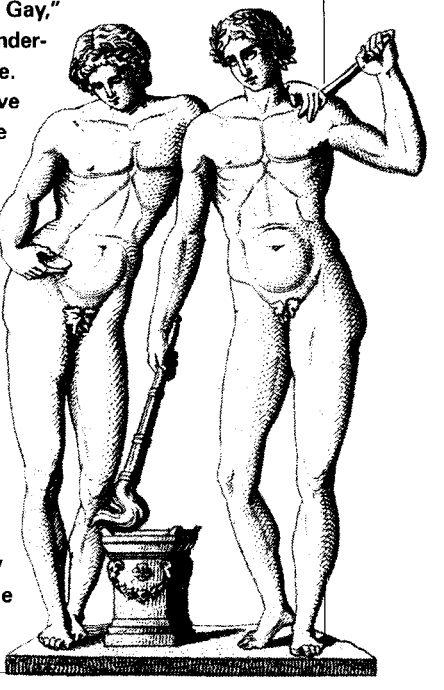
CHRISTOPHER SHEA

FEAR OF A QUEER CLASSROOM

FOR A COURSE TITLE, "HOW TO BE GAY" IS PROVOCATIVE. So is the description that University of Michigan English professor David Halperin wrote for it: "The course itself will constitute an experiment in the very process of initiation that it hopes to understand."

"It kind of pushes the envelope," state tourism director Suzy Avery told the *Michigan Daily*. Indeed, Halperin seems to have exposed his university's flak catchers to serious mau-mauing. Avery is a former chairwoman of the Michigan Republican Party and now a candidate for the University of Michigan's Board of Regents. In "How to Be Gay," she and fellow candidate Wendy Anderson have found a campaign issue.

Sitting regent Dan Horning drove two and a half hours to attend the class's opening lecture, during which Halperin clarified his intent: "When I speak about the way men learn to be gay, I'm not referring to the way men become homosexual. I'm speaking about the way men who already are homosexual acquire a social identity as gay men." Nonetheless, Horning declared himself "offended." Was it the prospect of watching *Gypsy* for credit? To date, the university president, and the majority of regents now in office, have stood by Halperin's class.



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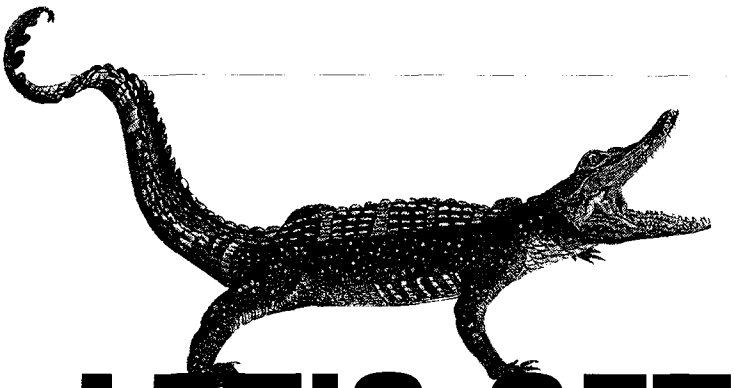


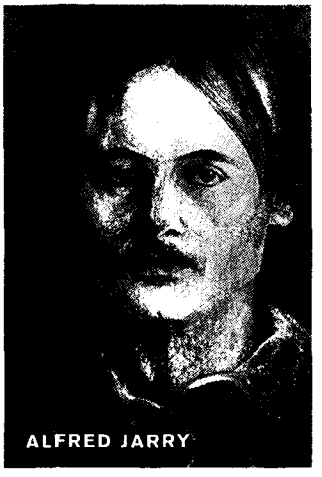
LET'S GET PATAPHYSICAL

THIS SPRING WAS A MOMENtous one for French letters. In May, the venerable Académie Française, which for years has been working on the newest edition of the official French dictionary, released its latest installment: *lésine* (stinginess) to *logogriphe* (logogriph). But this doubtless invaluable achievement was all but overshadowed by an event that took place the previous month: the first public appearance in twenty-five years of the Académie's institutional antithesis, the illustrious and outlandish Collège de 'Pataphysique.

On April 20, the famously reclusive cultural society threw itself a coming-out party in a windy garden near the Moulin Rouge. To judge by the proceedings, a quarter century underground had done little to dampen the group's legendary absurdist esprit. After listening to Gregorian chants and a harangue in Latin (titled "Collegii Pataphysici disoccultatio"), the pataphysicians were invited to pay their respects to a large black-and-white photograph of His Magnificence Lutembi, the Ugandan crocodile that has served for the last three years as their executive director.

Don't let the crocodile confuse you. Pataphysics is serious stuff, although just what it is is harder to say. The word "pataphysics" has been in circulation for more than a hundred years, ever since a teenage Alfred Jarry and his high school classmates in Rennes used it in the 1880s to mock their incompetent physics teacher. High school satire turned into a proto-Dadaist play, *Ubu roi,* and the physics teacher became Father Ubu, Jarry's bumbling antihero and history's first official pataphysician. To Jarry, the term denoted a scientist of the absurd, a theorist of "imaginary solutions," a practitioner of "logic as irrefutable as that of a madman or a piece of cake." Pataphysics, he proclaimed in 1898, would be to metaphysics what metaphysics is to physics.

It was an ambitious formulation. No wonder it took fifty years for pataphysics to find institutional backing. The support came in the form of an eighty-four-year-old lawyer and friend of Jarry's named Irénée-Louis Sandomir, who, with three associates, founded the Collège de 'Pataphysique in a Paris bookstore on May 11, 1948. (The apostrophe, the Collège decreed, would distinguish conscious 'pataphysics, that which one *does,* from unconscious pataphysics, that which one *is.*) Under Sandomir, who took the title vice curator founder, the Collège devised an elaborate internal hierarchy as well as a private calendar that began with Jarry's birthday and featured a thirteen-month year with regularly scheduled imaginary days. The Collège also began publishing a quarterly journal, which devoted many issues to Jarry's work. By the late 1950s, an impressive number of major postwar European writers and artists—including Eugène Ionesco (the society published the first edition of *The Bald Soprano*), Raymond Queneau,



ALFRED JARRY

Jacques Prévert, René Clair, Boris Vian, Max Ernst, Joan Miró, Marcel Duchamp, and Jean Dubuffet—had become pataphysicians.

Under Sandomir, the Collège functioned more or less in the public eye. As Ruy Launoir, the Collège's regent of *cléidologie,* recently explained in the pages of *Magazine Littéraire,* this openness occasionally led to "conflictual relationships with personalities from the vernacular world." In 1957, for example, upon learning that Sandomir had died, the prominent French editor Jean Paulhan expressed doubt that the legendary vice curator had ever really existed. The Collège struck back with a "pataphysical act of retaliation": It declared Paulhan himself a work of fiction. To impress the point upon French citizens, pataphysicians dispersed through Paris armed with cards that read "Jean Paulhan does not exist."

On the whole, however, the Collège avoided street actions, preferring the more sedentary avant-gardism of its *ouvroirs* (workshops). Instituted as subcommittees of the Collège, they were dedicated to different creative media. At least one of the *ouvroirs* took on a life of its own: By the late 1960s, Oulipo, the "workshop of potential literature," had become better known than its pataphysical progenitor. Under its auspices, Georges Perec published *La disparition* (*A Void*), a novel written entirely without the letter *e*. Other *ouvroirs,* including Oumathpo (a mathematics workshop), Oupeinpo (painting), Oumupo (music), Oubapo (cartoons), Oucuipo (cooking), and Ouphopo (photography), fared less well, expiring after just a few years. Then in 1975, for reasons murky even to its members, the Collège resolved to disappear for exactly twenty-five years. (Perhaps the Collège believed its worldly mission had been accomplished. After all, thanks to the 1969 Beatles song "Maxwell's

After twenty-five years, Alfred Jarry's underground college has resurfaced—led by a crocodile.

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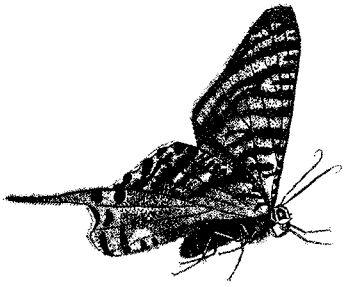
Silver Hammer," uncomprehending teenagers all over the world had been introduced to "pataphysical science.")

So now that the Collège is back aboveground, can the vernacular world look forward to a full-fledged pataphysical renaissance? With members like Fernando Arrabal—a well-known playwright, filmmaker, painter, poet, novelist, and chess expert—the Collège is looking more pataphysical than ever these days. Not so fast, cautions Launoir, writing in *Magazine Littéraire*: "The twenty-first century will be pataphysical or it won't be."

EMILY EAKIN



WINGED WORDS

IN AN AUGUST ISSUE OF THE moderated e-mail list LINGUIST, Brown University anthropologist William O. Beeman addressed an odd phenomenon: Apparently, there is a different word for butterfly in every language, even though historical relationships and geographic contacts often suggest the words should be similar. Beeman called it "the butterfly problem."

"We expect to find cognates," Beeman says, "but here we don't. What seems to happen is that groups come up with a term that is particular to them." By contrast, words for cat resemble each other closely in most Indo-European languages. "Moth" is also frequently borrowed. What about "bat"? "I know," Beeman concedes. "It's like eating peanuts—once you find out about one word, you want to go off and find out about more."

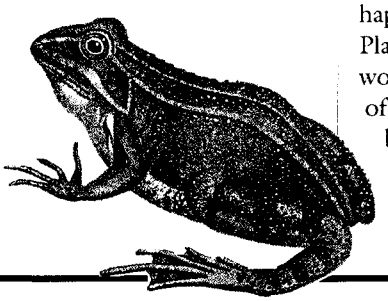
Beeman says his interest in butterflies began in an idle chat with mathematician friends. After a query to the LINGUIST list elicited fifty replies in two days, his involvement ballooned into a full-scale study. Beeman soon discovered he wasn't the first linguist to notice the phenomenon: In an unpublished 1982 paper, University of North Texas linguist Haj Ross rhapsodized about butterflies as such "perfect symbols of transformation" that "each language finds its own verbal beauty to celebrate the stunning salience of the butterfly's being." (Ross credited linguistic anthropologist Emmon Bach with first having first observed the phenomenon.)

In his post to LINGUIST, Beeman provided a list of words for butterfly in about ninety-seven languages, including Gujarati (*popti*), Lao (*maingkabula*), and Masai (*osampurumpuri*). But even if the words don't sound alike, are they as different as they seem? Beeman noted that a large number have one thing in common: They are related to "cultural metaphors." For example, the folkloric belief among Russians that butterflies are witches seems to be responsible for their butterfly word, *babochka*, or "little grandmother." The English "butterfly" and the German *Schmetterling* derive from the insect's supposed attraction to cream.

More important, many butterfly words share features such as the repetition of syllables (as in Malay *ramarama*, Senegalese *lupe lupe*, and Welsh *pili pala*). These sound patterns, Beeman provocatively argued, echo the symmetrical structure and movement of butterfly wings. Many of the words also repeat their consonants. "In...the many cases of reiterated *b*'s, *p*'s, *l*'s, and *f*'s (in widely separated language families)," Beeman wrote, "one can almost hear the gentle rustle of butterfly wings and see their repetitive motion."

Until a few years ago, Beeman's appeal to sound symbolism—the idea that certain sounds are naturally connected to certain meanings—would have been unfashionable, perhaps even derided. Ever since Plato's *Cratylus*, people have wondered whether the form of a word might be motivated by its content. But scientific debate on the question all but ended in 1916, with the posthumous publica-

Does every language have a different word for butterfly? "We expect to find cognates," says anthropologist William Beeman, "but here we don't."



TO NIETZSCHE'S CREDIT

IS FRIEDRICH NIETZSCHE A DANGER TO SOCIETY? EVER SINCE convicted murderers Nathan Leopold and Richard Loeb confessed their enthusiasm for the German aphorist, the question has returned eternally. Now, it seems, the philosopher can claim yet another disciple whose extra-moral sense is sharp indeed.

This August, Professor James Cadello of Central Washington University was sentenced to six months in prison after he pleaded guilty to the crime of "identity theft." The author of a dissertation on Nietzsche's early reception in the United States, Cadello taught well-received classes on Continental philosophy and postmodernism and received tenure in 1999. According to the *Seattle Times,* he began his career in forgery when he helped a nineteen-year-old student acquire a fake ID so they could drink together when they went to bars. Soon afterward, Cadello obtained the names and Social Security Numbers of Central Washington students from the university's computer system. Using that information, he invented a number of fictitious individuals and went about obtaining identification and credit cards in their name. The professor never did use the credit cards, but authorities were still not amused when they discovered no fewer than forty fake IDs in his house.

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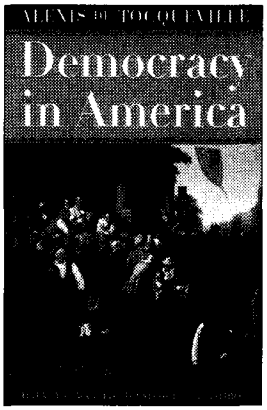

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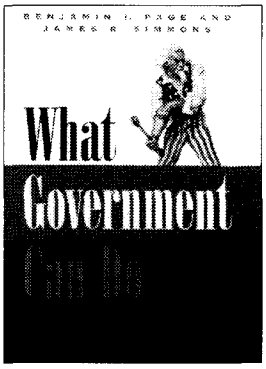

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Creature names in Japanese movies may be instances of sound symbolism: The syllable /ra/ seems to represent large size and monstrousness, as in Mothra or Godzilla.

tion of Ferdinand de Saussure's *Course in General Linguistics*. Saussure declared that linguistic signs were conventional and arbitrary. "There is no internal connexion, for example, between the idea 'sister' and the French sequence of sounds s-ö-r [*soeur*] which acts as its signal," he wrote. According to Saussure, if a word such as "cuckoo" mimics a cuckoo's call, it's no more than coincidence. The few genuine onomatopoetic words that do exist—such as *glas*, the French word for knell—Saussure dismissed as marginal cases. By midcentury, Saussure's theory had become linguistic dogma.

"Sound symbolism is hard to study," says Leanne Hinton, professor of linguistics at Berkeley. "It's easier to study things that have definite rules to them, something that's more definitely patterned or governed in some predictable way." Hinton believed that Saussurean dogma needed to be challenged, and so, with fellow linguists Johanna Nichols and John Ohala, she co-edited *Sound Symbolism* (Cambridge) in 1994. Unfortunately, Hinton says, "a lot of people have cockamamy ideas about sound symbolism, so it gets a bad reputation."

"In the back of every linguist's mind, there's always been some question about sound symbolism," says Beeman. He points to a 1929 experiment by Edward Sapir in which Sapir's subjects were asked to match nonsense words with small and large versions of the same object. The subjects tended to match words with a high vowel (such as *ee*) to the small object and those with a low vowel (such as the *o* in "cot") to the larger object. British linguist J.R. Firth later called these links between sound and meaning "phonesthemes." One English phonestheme, the consonant cluster /gl/, is generally taken to mean "light" or "shining," as evidenced in words such as "glass," "glisten," "glow," "glare," "glimmer," "glimpse," "glitter," and "gloss." (The English poet Gerard Manley Hopkins loved to compile lists like this in his journals.)

"These things are fun, but very few people want to put in the work to go about it in a rigorous way," says Terence Hays, an ethnobiologist at Rhode Island College who has tackled what one might call the "frog problem": The sound /r/ or /g/ recurs in over two hundred words for frogs and toads in New Guinea. (John Beatty of Brooklyn College has also play-

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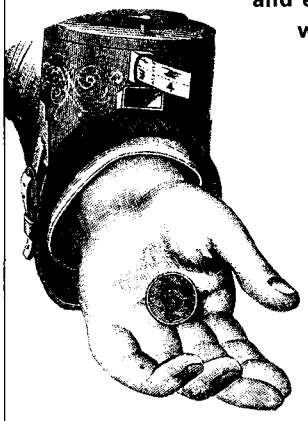
THE GAMBLING MAJOR

IT SOUNDS LIKE A FAMILIAR STORY: STATE LEGISLATORS contest new additions to the higher education curriculum. But this time, it's not evolution or queer theory that's in dispute. Mississippi's gaming industry, which operates thirty casinos and employs forty thousand people, wants colleges to offer courses on such topics as purchasing, security, management, and slot-machine maintenance, *Community College Week* reports. "We are not talking about how to teach people to play blackjack," insists Andy Bourland, executive director of the Mississippi Gaming Association. Rather, the courses would be geared toward training future employees of Mississippi's $3-billion-a-year business. But although bills allowing such courses have passed the House, they've died in the Senate for each of the past three years. Their most implacable foe is the Mississippi Baptist Convention. Says Republican representative Richard White, "They will oppose it forever. It is a sin."



fully suggested what might be called the Godzilla problem: The syllable /ra/ seems to symbolize large size and monstrousness when used to name creatures in Japanese movies.)

Unfortunately, to establish the sound symbolism of butterfly terms to a linguist's satisfaction would require a herculean effort, Hays says. After all, the repetition of syllables and other elements is a common feature of many languages. "Only if you show me a language that never reduplicates sounds in nouns but does it with butterflies, then I'll sit up and listen," he says. After he read Beeman's post, Hays consulted his database of the Mailuan language family in New Guinea. Among the five words for butterfly in the Mailuan languages, he found two cognates.

But Hays doesn't discourage the search for sound symbolism altogether. He cites work by ethnobiologists such as Brent Berlin, who has found that in Huambisa, a language spoken in northern Peru, some vowels are more often associated with elongated animals than with flying animals. "So there are some interesting things going on with sound symbolism," Hays says.

"Actually, if you go to the field," he adds, "you notice fireflies most of all. Here are spots of light, appearing, disappearing, all around you. They're much more salient than butterflies." But there's not likely to be a firefly problem: In the languages Hays studies, the word for firefly is almost always the same as that for star. Explains Hays: "Just because something really strikes you perceptually doesn't mean you'll treat it in a special linguistic way."

MICHAEL ERARD

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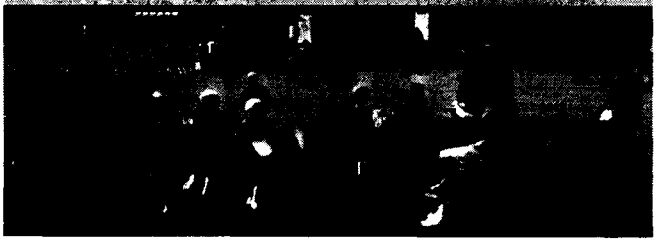

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biographies, and satires, Fitzgerald examines competing ideologies of servility in Rome and the ways that Roman slaves, as imagined by their masters, mirrored and mediated relations among the free. ***Life, Death, and Entertainment in the Roman Empire*** (Michigan, 1998), edited by **David S. Potter** and **D.J. Mattingly,** offers informative articles on other aspects of everyday life in ancient Rome, including religious ritual, the family, and elite-male identity. The contributors ask relevant questions about the political dimensions of Roman leisure activities and provide lively discussion where one would expect heavy going, such as in essays on demographics and food supply. The collection also provides scholarly correctives for thousands of misled *Ben-Hur* and *Gladiator* fans: It was very rare for charioteers to win by coming from behind, and most gladiatorial bouts ended without a kill."

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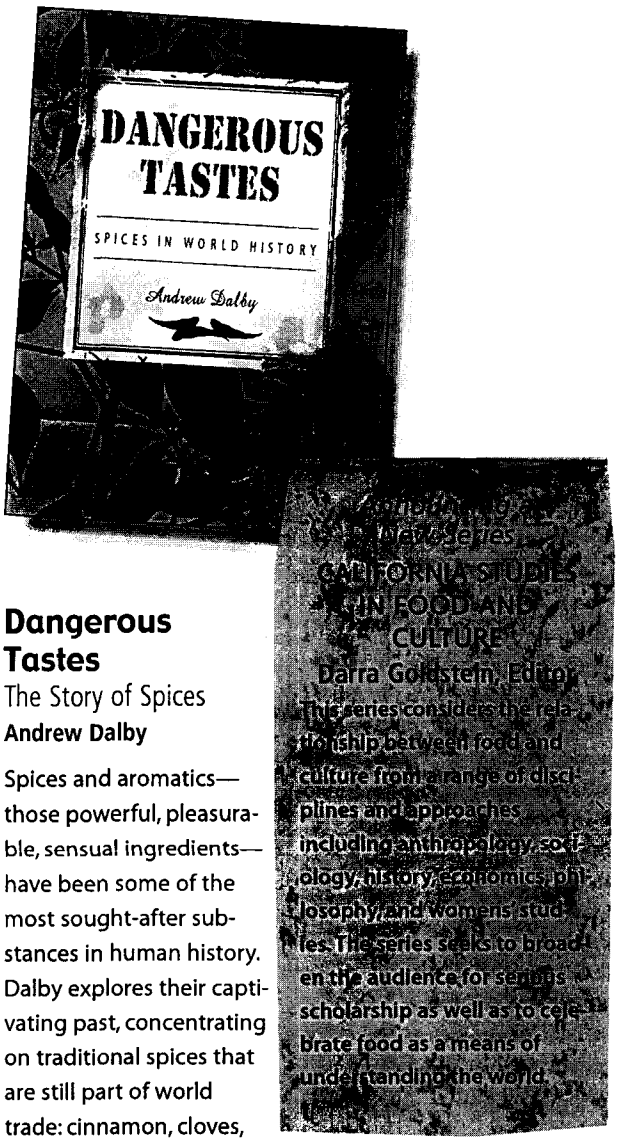
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describes how the emperor Augustus codified traditional attitudes toward prostitutes and pimps into common law. Although this book gets into the nitty-gritty of the sex industry only here and there, McGinn's forthcoming study, *Prostitution and Roman Society*, will no doubt have much more to say on the subject, which has so far escaped systematic treatment in English."

Keith Hopwood, lecturer in classics at the University of Wales, Lampeter, and editor of *Organised Crime in Antiquity* (Classical Press of Wales, 1998).

"Unorthodox in format but brilliant, **Keith M. Hopkins**'s ***A World Full of Gods: The Triumph of Christianity*** (Free Press, 2000) attempts to recreate the multitudinous faiths of the Roman Empire. Hopkins sends specially commissioned time travelers to Pompeii and investigates post-Temple Judaism in the format of a British news program. Scholars both real and imaginary send ripostes to Hopkins's idiosyncratic approaches. The mélange sheds new light on the rise of Christianity and exposes some unpleasant truths about the origins of anti-Semitism. Meanwhile, **Stephen C. Todd**'s ***The Shape of Athenian Law*** (Oxford, 1995) elucidates a key aspect of Athenian democracy. Using cutting-edge anthropological models, Todd devotes a chapter to each aspect of the Athenian legal system and introduces each chapter by presenting a typical case. Todd ultimately sees the courts as an extension of democracy: The random collection of citizen-jurors voted on legal issues and the definition of the law, as well as on the case before them."

James Davidson, professor in the School of History, Classics, and Archaeology at Birkbeck College, London, and author of *Courtesans and Fishcakes: The Consuming Passions of Classical Athens* (HarperCollins, 1999).

"In recent years, first-rate scholarship has done much to situate ancient Greek art in its proper cultural context. Although there are many excellent titles, and it seems invidious to recommend only one, **Robin Osborne**'s ***Archaic and Classical Greek Art*** (Oxford, 1998) stands out for its range and cogency. Osborne shows, for example, how statues of naked men functioned at once as sexual objects, as ideal images of humanity, as memorials to the dead, and as gifts to the gods. He shows how ancient Greek vases evoke a lost world of drinking parties and socializing, and he demonstrates how temples reflect Greek ideas about cosmic order and the place of the Greek city within that cosmos. Meanwhile, **Helen King**'s ***Hippocrates' Woman: Reading the Female Body in Ancient Greece*** (Routledge, 1998) surveys a broad range of ancient gynecological texts from Hippocrates to Galen and shows how they have been used and misused right up to the twentieth century. In the classical Greek belief system, women's wombs could be tamed by applying noxious substances to the mouth or sweet-smelling ones to the genitals. A woman whose husband had been exiled would grow a beard. King mixes amusement at this strange pseudoscientific world with astonishment that ineffectual remedies continued for as long as they did."

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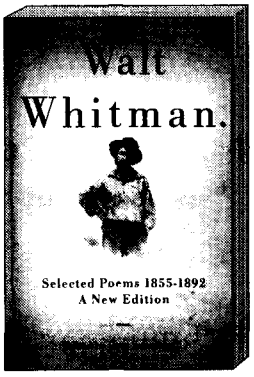

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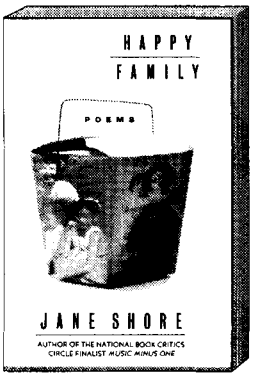

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Tom Stoppard

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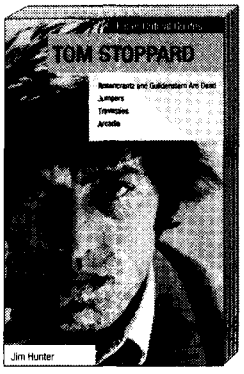

Hunter mines four major Stoppard plays in order to acquaint readers with the distinctive aspects of this playwright's work. Stoppard's importance to today's theatre is also discussed, as are the language, structure, characters, themes, and performance features of his drama. "In a Stoppard play we can expect to find three particular elements: brilliant language, absurd yet inspired theatrical ideas, and an intellectual frame of reference which is not mocked, whatever else is."—Jim Hunter, from his Introduction

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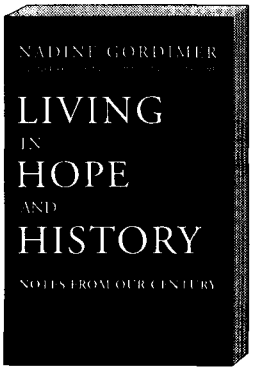

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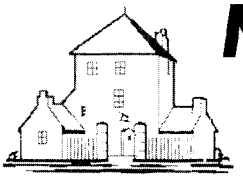

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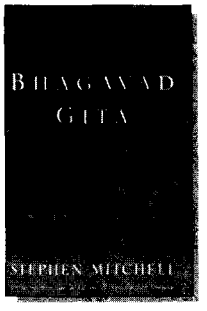

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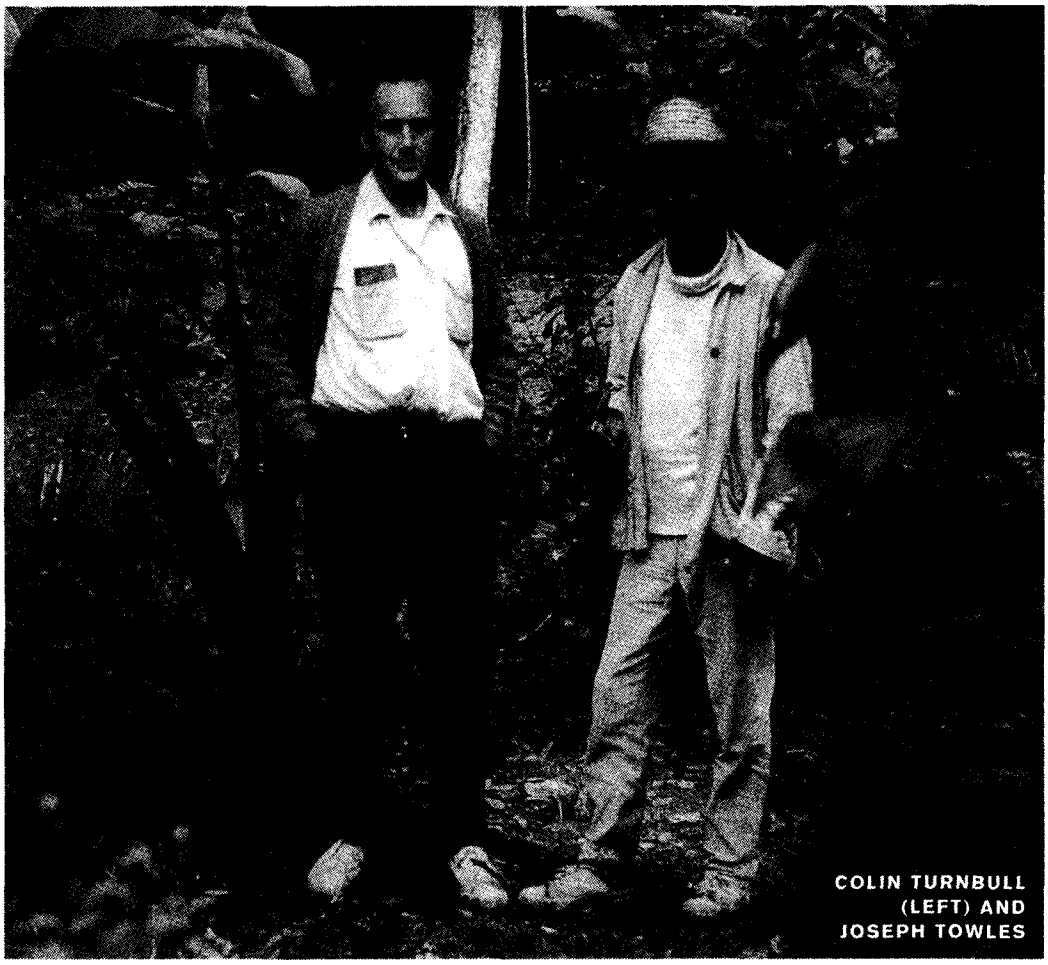

COLIN TURNBULL (LEFT) AND JOSEPH TOWLES

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THE ANTHROPOLOGIST AS LOVER

ROY RICHARD GRINKER WAS A COLLEGE freshman when he first read Colin Turnbull's anthropological blockbuster *The Forest People,* a romantic portrait of the African Pygmies. "I loved it!" he recalls. "It transported me to another part of the world." But by the time Grinker was studying the Pygmies himself, he, like many of his peers, had come to view Turnbull as an outdated embarrassment—a simplistic popularizer who relied on poetic moral judgments instead of reasoned analysis. "The work seemed so barren of hypotheses I could test, barren of theories," recalls Grinker. "I wanted to throw *The Forest People* across the room!" He laughs ruefully. "I was *mad* at him."

Grinker was angry enough to ignore a generous letter Turnbull wrote, offering to aid him with his research. And when Grinker published his own ethnography, *Houses in the Rainforest: Ethnicity and Inequality Between Farmers and Foragers in Central Africa* (1994), the book energetically debunked Turnbull's central thesis: that the forest-dwelling Pygmies were a separate culture from the villagers with whom they traded. Turnbull, wrote Grinker, didn't understand the community because he didn't have the tools to do so—right down to the Pygmy language itself.

It was a classic case of an academic Oedipal conflict. So how did Grinker come to write *In the Arms of Africa* (St. Martin's), a sympathetic

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Grinker had come to view Turnbull as an outdated embarrassment. Then he got hold of Turnbull's personal journals.

and deeply moving biography of the precursor whose work he had once reviled? Repentance, for one thing. "I had failed to meet a remarkable person because I was consumed by youthful narcissism," he writes in his acknowledgments. There were other motives as well. While occupying Turnbull's position at George Washington University, Grinker found himself surrounded by Turnbull's former colleagues. One of them, Bob Humphrey, was the anthropologist's executor. And as boxes of papers and journals poured into the anthropology department following Turnbull's death in 1994, Humphrey encouraged Grinker to look at them. Grinker discovered a man of courage and originality, as well as a spiritual seeker who had maintained a passionate and committed interracial gay relationship despite overwhelming social pressures.

The resulting memoir is a love letter, but a nuanced one. Turnbull comes across as brilliant, sensitive, and charismatic, yet often cruel and distant; a scholar passionately devoted to social justice yet blind to his own power. He loved the Pygmies and painted them as the essence of goodness, but he hated the Ugandan Ik (the subject of his controversial second book, *The Mountain People*) and depicted them as purely evil. Ultimately, writes Grinker, Turnbull changed anthropology in crucial ways, largely through the force of his writing: "The truth of the Zairean rainforest or the tragedy of the Ugandan mountains could not be conveyed in an academic publication to be read by a few hundred scholars. It had to reach millions of people and to come from the heart."

Though Turnbull was born into a gruff Scottish-Irish family, he rejected much of his heritage, starting with his traditional British boys school. "I do not think I learned anything useful in that school," he wrote acidly, "except how to lie with only moderate success, how to cheat, and how to have zero confidence in adults and peers alike." Enamored of Eastern spirituality, after a stint in the navy Turnbull spent several years in an ashram. And he had a long and intimate love affair with an Indian woman, braving the snubs of his community when he brought her to a local dance.

But it was when he turned to anthropology that Turnbull found the perfect outlet for his desire to spotlight the ignored and underestimated. He presented the Pygmies as loving, peaceful, and close to nature. They raised children with love, not discipline. Their sexuality found its place on a continuum of pleasure that encompassed the glimmer of the moon and the rhythms of their native instrument, the molimo trumpet. Their seeming submission to the Bira villagers in fact concealed a clever subversion: They pretended to go along with their oppressors' religious taboos but privately maintained a cheerful independence and benefited from the exchange of goods. This analysis may have been too simple, Grinker argues, but it was a powerful expression of Turnbull's lifelong message: that Western imperialism brought about a deep spiritual sickness, and that Indian and Asian cultures offer a better vision for humanity.

According to Grinker, the central experience of Turnbull's life was his thirty-year love affair with the younger African-American man whom Turnbull called "my wife, Josephine." Grinker argues that Turnbull viewed his relationship with Joseph Towles as a metaphor for his relationship with Africa: Turnbull saw his young protégé as a neglected genius, and he was determined to reverse their positions in the eyes of the world. Indeed, the final years of Turnbull's life were spent in a desperate attempt to cement Towles's reputation as the superior anthropologist.

Towles and Turnbull had a tempestuous union, and Turnbull could be controlling and cruel. Nonetheless, the couple's bond withstood remarkable pressure, including harassment from Turnbull's scholarly peers. When Turnbull worked at New York's Museum of Natural History during the early 1960s, he brought Towles to department parties. In response, an openly xenophobic colleague, Jim Ford, sought to hoax Turnbull by sending him a series of fictional letters purporting to offer valuable tribal artifacts. Finally, Ford wrote a public letter to the anthropology department that denounced Turnbull's homosexuality and referred to Towles as his "private specimen." Turnbull's dignified reaction when the screed was passed around at a faculty meeting so impressed his colleagues that Ford was fired shortly thereafter.

Turnbull eventually retired and began focusing much of his attention on Towles's troubled academic career at Virginia Commonwealth University and elsewhere. He also turned toward social justice issues, including research and advocacy work on death row. The wrenching ending of Grinker's book recounts Towles's descent into mental illness and AIDS-induced dementia as Turnbull patiently nursed him at their home in rural Virginia. After his lover's funeral, Turnbull retreated from the world entirely, refusing medical treatment. In his final days, he became a Buddhist monk, retiring to an ashram in Bloomington, Indiana—only to be ejected when, Grinker suspects, his hosts discovered he had AIDS.

Unlike Grinker's earlier work, *In the Arms of Africa* is no scholarly treatise. Passionate and often

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erotic, it is a page-turner, much like *The Forest People* itself. Grinker is not blind to the irony. "Hey, I was jealous!" he acknowledges with endearing candor. "I saw syllabi using Turnbull's book—and I wondered, 'Why can't they have *my* book on the Pygmies?'" But making the emotional leap into Turnbull's life story helped Grinker reevaluate what it meant for a writer to have a cause rather than a thesis. "I understood then the motivation to think of who your audience was, who could share what you felt rather than just what you learned," he muses. "I recognized that Turnbull wasn't trying to do what I was trying to do. He was trying to convince people that truth and beauty existed—that those values were there, and that they existed where people thought they didn't."

EMILY NUSSBAUM

ON THE GENEALOGY OF MORELS

SINCE NEW YORK UNIVERSITY INTRODUCED the first doctoral program in food studies four years ago, the field has had trouble achieving academic respectability. Some critics accuse food scholars of making a naturally juicy topic dry and unappetizing, while others worry that their interdisciplinary efforts lack rigor. Food studies may yield a new harvest this fall, however, thanks to an unlikely partnership between the fledgling discipline and university presses seeking to capitalize on popular interest in food and dining.

This month, the University of California Press will launch California Studies in Food and Culture, an ambitious, open-ended series on culinary history. The first title of the series, Andrew Dalby's *Dangerous Tastes: The Story of Spices,* traces the changing fashions, economics, and belief systems that have surrounded aromatics from cinnamon and coriander to ambergris and aloeswood. Subsequent titles, following at a rate of one or two per season, will cover topics ranging from the politics behind the Food and Drug Administration to Renaissance dietetics and the history of Camembert.

Over the last six months, Illinois, Harvard, Johns Hopkins, and Pennsylvania have also published serious books about food and dining. Illinois, like California, committed to an ongoing series on culinary history this past April, and South Carolina considers the subject one of its specialties. Bearing little resemblance to M.F.K. Fisher's ruminative essays on the cooking life, the new scholarly food books examine everything from the rise and fall of French haute cuisine to the social history of popcorn.

The sudden interest, however, may be as much economic as intellectual. In today's culture of weekend gourmets, culinary chic, and skyrocketing cookbook sales, university presses increasingly see books about food as a way of reaching coveted nonacademic readers. David Perkins, assistant director at the University of Illinois Press, points to a ballooning market of highly educated, upper-middle-class Americans with a voracious appetite for culinary literature of all kinds. "Foodies are very serious people," says Perkins. "When they're not cooking or eating food, they enjoy reading about it. They like looking at antique cookbooks, reading about the history of table manners—just about anything."

Food studies scholars are delighted by the attention. Andrew F. Smith, a freelance culinary historian and editor of Illinois's food series, says the recent publishing boom makes him feel like "a kid walking into a candy store." Darra Goldstein, a professor of Russian at Williams College and editor of the California series, shares Smith's excitement. "Food studies is in the same position today that women's studies or film studies was thirty years ago," she says. "It's a topic on the margins that's about to come of age."

If it does, Goldstein will have played no small part in the field's rite of passage. In addition to editing California Studies in Food and Culture, Goldstein serves as a consultant to New York's Russian Tea Room, where she attempts to ensure that the fashionable restaurant doesn't stray too far from the culinary traditions of the czarist aristocracy. She has also signed on to edit *Gastronomica,* a glossy "journal of food and culture" that California will debut this February. Billing itself as a "crossover publication," *Gastronomica* will contain belletristic essays, commissioned artwork, and original poetry, as well as scholarly research. The first issue includes a brief history of the *olla* (a Mediterranean clay pot), a symbolic analysis of foods depicted in seventeenth-century Dutch still lifes, and original con-

"Foodies are very serious people," says Perkins. "When they're not cooking or eating food, they enjoy reading about it."

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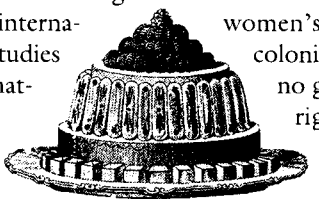
"I want to know about the famines as well as the feasts," laments Kaplan.

tributions by poet Louise Glück and *New Yorker* cartoonist J.B. Handelsman. With its full-color photographs and chic graphic design, the journal has more in common visually with *Artforum* than with competing academic quarterlies.

Has California's market ambition constrained its academic vision? Steven L. Kaplan, professor of history at Cornell University and founding editor of *Food and Foodways,* an international journal that pioneered food studies in the mid-1980s, worries that whatever hard-won legitimacy the field has achieved over the years will be lost if readers begin to conflate more serious scholarship with the work of armchair enthusiasts and popular historians. "It's lamentable if the only way to sell food studies is by focusing on the glossiest, sexiest aspects of food," argues Kaplan, pointing to *Gastronomica*'s name as evidence of its limited approach. "Gastronomy, by definition, is the art and science of good eating, and to confine oneself to the kitchen, and the high kitchen at that, impoverishes the discipline. It's sexy and it's jazzy and it looks good, but 90 percent of food studies deals with the banality of eating—the economic dimensions of food, the political dimensions of food, the religious dimensions of food—things that have nothing to do with Michelin three-star restaurants. I want to know about the famines as well as the feasts."



To be sure, *Gastronomica* bears some unmistakably commercial touches. Along with historians and sociologists, the journal's editorial board includes an upscale Sacramento food and wine merchant, *Washington Post* restaurant critic Phyllis Richman, and one man identified only as "the curious cook." Promotional literature features endorsements from chef Alice Waters and celebrity television gourmets Jacques Pépin and Anne Willan, although none from established scholars in the field. Goldstein is adamant in her desire to keep the publication jargon-free: "I don't want to publish articles that don't reach out beyond the scholarly community," she insists. But she maintains that quality of scholarship will remain the primary criterion for article inclusion. "I guess I want everything," she says with a laugh, "the perfectly enjoyable article that's also perfectly researched."

Many of Goldstein's colleagues will be pleased even if the journal delivers a good deal less. "I think *Gastronomica* fills a niche that's not currently being filled," says Amy Bentley, an assistant professor of food studies at New York University. "Those who focus on the production of food and on the politics of sustainable agriculture often forget about the aesthetics of food, the culture of food, and the enjoyment of food." Kaplan himself concedes that some in the field have called his journal "narrow," or even "stuffy."

In many ways, *Gastronomica*'s ability to stir up dissent may prove its greatest accomplishment. After all, the very tameness of food studies has contributed to its marginal status. Unlike women's studies, queer studies, or postcolonial studies, food studies generates no great polarization between left and right and poses no uncomfortable challenge to the established social order. Even food historians' most politically charged argument—that decades of domestic stereotypes have discredited cookbooks as legitimate historical documents—strikes many observers as bland. Faced with the challenges of establishing a new discipline, food studies scholars have rarely opted for serious self-criticism. And if *Gastronomica* changes nothing more than that, it will have accomplished enough.

JASON SHOLL

JOHN CLARE

FREE AND CLARE

AFTER THE 1820 SUCCESS OF HIS *POEMS Descriptive of Rural Life*, John Clare was thrust into the London limelight. The diminutive and self-taught Clare, who had previously worked as

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a plowboy and thresher in rural Northamptonshire, hobnobbed for a time with the likes of Charles Lamb and William Hazlitt. But by the time he died at Northampton General Lunatic Asylum in 1864, Clare had been largely forgotten. Only one of his poems made it into Arthur Quiller-Couch's influential *Oxford Book of English Verse* in 1900, "I Am," a work eerily prophetic of its author's fate: "I am—yet what I am, none cares or knows; / My friends forsake me like a memory lost."

Today, however, the chronically poor, painfully shy Clare is often included in the company of Wordsworth, Keats, and Shelley. And his high reputation is due in no small part to the efforts of retired University of Massachusetts at Boston history professor Eric Robinson, who edited the standard edition of Clare for Oxford University Press. Robinson has devoted much of his professional life to this massive and forbidding (and expensive—some volumes cost $180 apiece) nine-volume edition. But Robinson is not just an editor: He also claims to own the copyright to much of Clare's poetry, and he now stands accused of trying to thwart a young scholar's attempts to edit Clare.

In a July 2000 letter to the *Times Literary Supplement*, twenty-eight scholars and poets—including the English poet laureate Andrew Motion, the Nobel Prize winner Seamus Heaney, the biographer Janet Todd, and the psychoanalyst and critic Adam Phillips—rallied to the cause of Simon Kövesi, a twenty-eight-year-old teaching fellow in English at the University of Dundee. In July 1999, Kövesi had published *John Clare: Love Poems* with M&C Services, an imprint based in Bangkok and run by publisher Michael Gorman. Several months later, Robinson's lawyers charged Kövesi with copyright infringement and demanded financial redress. Kövesi has refused to pay up. In fact, he has edited another work, *John Clare: Flower Poems*, slated for October. "I will not pay someone who has not done work on my book," Kövesi says bluntly.

At stake is not just money or literary property. There's also a difference of opinion on whether to standardize Clare's English, which was fairly unorthodox in spelling and punctuation. Robinson prefers to publish his verse in its rough-hewn original form, idiosyncrasies intact. One Clare scholar calls this "unediting," and Zachary Leader, the author of *Revision and Romantic Authorship* (Oxford, 1996), has dubbed it "textual primitivism." Kövesi feels that today's

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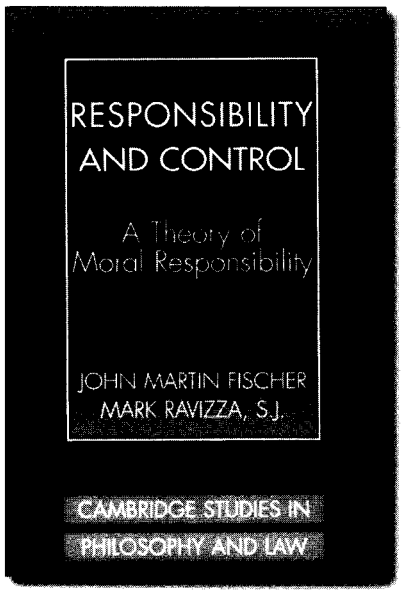
This book provides a comprehensive, systematic theory of moral responsibility. The authors explore the conditions under which individuals are morally responsible for actions, omissions, consequences, and emotions. The leading idea in the book is that moral responsibility is based on "guidance control." This control has two components: the mechanism that issues in the relevant behavior must be the agent's own mechanism, and it must be appropriately responsive to reasons. The book develops an account of both components. The authors go on to offer a sustained defense of the thesis that moral responsibility is compatible with causal determinism. This major study will interest moral philosophers, legal theorists, and those in religious studies concerned with the issue of moral responsibility.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In 1965, Robinson purchased the copyright to much of Clare's work for only one pound.

nonscholarly readers should be free to choose a more accessible text.

How could a professor own the copyright to work by an English poet dead for 136 years? It's a complicated story. Clare left behind some three thousand unpublished poems; under English common law, authors (or their legal deputies) retain control over unpublished writing indefinitely. So when Robinson started to put together his edition of Clare in the early 1960s, he sought out the rightful copyright owner. After a bit of sleuthing, he determined that the Clare estate had sold many of Clare's manuscripts to a London publisher named Joseph Whitaker in 1864. Whitaker later sold most of the manuscripts, but in selling them he did not surrender the copyright—a distinction Robinson has been keen to make, because in 1965 he bought that copyright for merely one pound from Whitaker's publishing house. As Robinson boasted to the *Times Higher Education Supplement* last year, his purchase was a "brilliant piece of entrepreneurial effort"—a remark Robinson's critics have seized on as evidence that he is more concerned with profit than with literary inquiry.

Kövesi has cast doubt on the historical basis of Robinson's copyright claim—Whitaker's original purchase of the Clare copyright. Because Clare was legally insane for the last twenty years of his life, copyright to his work was actually in the hands of three trustees: a close friend and former publisher named John Taylor and two other men. Although Whitaker claimed that he and Taylor made "a provisional bargain" soon after Clare's death, they probably never finalized the purchase, because Taylor himself fell ill and died six and a half weeks after Clare. Following Taylor's death, Whitaker seems to have negotiated with Clare's illiterate widow rather than with Clare's other two trustees. Kövesi alleges that Whitaker's purchase was thus "without the formal or legal authority of the trustees to Clare's estate." Unfortunately, it is impossible to establish the exact status of Whitaker's claim, because his records were destroyed during the Nazi blitz on London in 1940. But in the last decades of his life, Whitaker seems not to have defended his hold on the copyright against those interested in printing editions of Clare's work. As Kövesi points out, for more than eighty years no one asserted control over the work of John Clare.

Since his 1965 purchase, however, Robinson has defended his own claims aggressively. This has brought him into conflict with a number of

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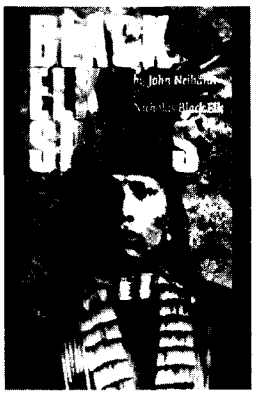

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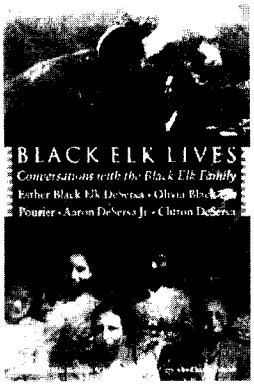

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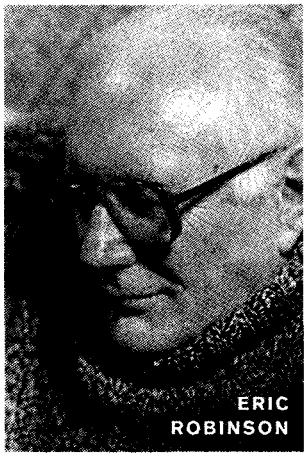
Clare enthusiasts, who note the irony of enforcing property rights to the work of a poet who was their adamant enemy. (Clare once wrote that nineteenth-century England's Acts of Enclosure "came and trampled on the grave / Of labours right and left the poor a slave.") In a July issue of the *Guardian*, Nottingham Trent University's John Goodridge wrote that he had "heard many tales of blighted projects" over the years. Goodridge, who supervised Kövesi's dissertation, reported that he himself met with "prevarication and delay" from Robinson's literary agent last year when preparing an edition of Clare's writings of 1841. The University of Glasgow's Janet Todd, a biographer of Aphra Behn and Mary Wollstonecraft, reports that she went head-to-head with Robinson in the early 1970s. According to Todd, when she wrote to Robinson proposing an edition, he refused permission unless they worked as collaborators—an offer Todd declined. Anne Tibble published a two-volume Clare edition with her husband back in 1935, but in the 1970s Robinson granted permission for her edition of Clare's *The Midsummer Cushion* only after long wrangling.



ERIC ROBINSON

For all that, Robinson is dismayed that he has been cast as the Goliath to Kövesi's David. "I'm the little fellow in this," he says. "It is the Kövesi-Gorman alliance that is powerful." As evidence of his willingness to tolerate alternative editions, Robinson points out that English law gives him the right to sue editors who failed to respect the Clare copyright before he bought it—a right he has declined to exercise. He also points out that Elaine Feinstein, Carolyn Kizer, and his accuser, Goodridge, have published editions of Clare since 1965 and that he even waived copyright fees for two editions prepared by Peter Moyse, the secretary of the John Clare Society. To the charge that he is motivated by greed, Robinson professes astonishment. "I did not expect to make much money out of [Clare's copyright] because there was hardly anyone reading Clare," he says. "I hadn't acquired somebody's film rights."

MATTHEW PRICE

Robinson prefers to publish Clare's verse in its rough-hewn original form. One critic calls this "unediting."

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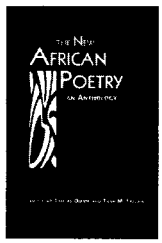

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CAN MARRIAGE BE SAVED?

AN UNSENTIMENTAL CASE FOR MATRIMONY

BY **ELISE HARRIS**

Marriage appears to be on the rocks. According to the University of Chicago's National Opinion Research Center, nearly 75 percent of American adults had spouses in 1972. In 1998, only 56 percent did. Since 1970, cohabitation by unmarried couples has increased sevenfold. Thirty-two percent of 1996 births were out of wedlock (the figure was 70 percent for African Americans). The divorce rate has stabilized at 50 percent.

To explain marriage's decline, some analysts (usually on the political left) point to large structural forces, such as the rise in women's employment and the lengthening of life spans. Others (usually on the right) deliver jeremiads against self-centeredness and the sexual revolution. Feminists promote government-subsidized day care, while Promise Keepers vow to restore men's stewardship of the family. At times the intellectual debate over marriage seems to have reached a stalemate, with each side repeating the same arguments over and over—much like a lousy marriage.

Now a respected sociology professor from the University of Chicago, Linda Waite, hopes to settle this tired old feud with a new claim. Her pragmatic argument: Marriage is a bargain, for both men and women, and few of us can afford to pass it up.

Waite, a quantitative sociologist and demographer, hopes that a public health argument will succeed where moral suasion has failed. She argues that the single life, like smoking, is bad for the individual and the community. And to ensure that her message will be heard, she has chosen a co-author with a talent for polemic: Maggie Gallagher, a conservative syndicated columnist at the Institute for American Values who has written several books on family issues. In *The Case for Marriage: Why Married People Are Happier, Healthier, and Better Off Financially* (Doubleday), Waite and Gallagher contend that marriage causes more positive effects (what sociologists call "good outcomes") than either living alone or cohabiting with a boyfriend or girlfriend. Their book relies not on browbeating but on math. The benefit of marriage, they assert, derives from economies of scale—two people living together for the cost of 1.6 people—and

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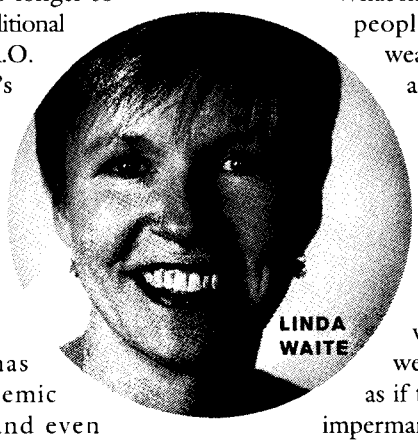
from the specialized division of labor within the household.

In its most time-honored form, specialization divides a couple's labor along gender lines: "He works for money, she tends to kids and kin." Waite claims that today's two-earner couples still divide their work, but more flexibly: "You do the insurance, I'll take the investments" or "You walk the dog, I'll clean the bathroom." As Adam Smith knew, specialization allows one to accomplish more. Waite has calculated just how much more by compiling other researchers' findings and by performing regression analyses to determine the impact of marriage on affluence, sexual satisfaction, mental health, physical health, and parenting. She and Gallagher cap their book off with a raft of policy ideas about how to keep marriages together.

Some critics argue that the apparent marriage benefit is a statistical artifact—the result of a self-selecting sample. After all, people who are already happy, healthy, and well adjusted are more likely to get married and stay married than those who are not. Others fault Waite and Gallagher's book for shying away from the controversial question of changing gender roles. Many attribute the rising divorce rate to women's increasing economic independence and desire for equality. Indeed, in an earlier book, Waite herself argued that marriage would only survive if it became more egalitarian, with men and women balancing household tasks more fairly. But in *The Case for Marriage*, she is no longer so avid about updating traditional gender roles. (In *Slate*, A.O. Scott called the book's characterization of male behavior "rather quaint.") Rather, she is worried that the rapid embrace of new family forms, such as single parenting, might be ill advised.



Though Waite has departed from academic liberal orthodoxy and even enlisted a conservative ally, her appeal to family values has a pragmatic, centrist bent. She combines her call for stricter divorce laws, for instance, with support for gay marriage. Will other women adopt her cool-headed conclusions? And once they're told the incentives for staying, can women—and men—learn to prefer flawed, actual unions to the ideal marriages they imagine?

WHEN WAITE AND I meet in a bagel shop in a Chicago suburb, she is wearing a suburbanite's white linen top, gray linen pants, and a green Gap baseball hat. She is direct and friendly, with a manner that is polite but slightly detached. Married to the sociologist Ross Stolzenberg, who also teaches at the University of Chicago, Waite is the mother of two daughters, aged twenty-one and fifteen.

Waite was chair of the American Sociological Association's family committee in 1999. She is also the co-author of a widely respected book, *New Families, No Families?* (California, 1991), an analysis of the factors that predict marriage and divorce. And she has recently edited a densely quantitative anthology on marriage and cohabitation patterns, *The Ties That Bind* (de Gruyter), published in June.

She has, in other words, been studying marriage for decades—decades when marriage puzzled sociologists by its steady falloff in prevalence and prestige. *The Case for Marriage* itself is an expanded version of a presidential address Waite gave to the Population Association of America in 1995. "I wanted to be more relevant—not just be an ivory-tower academic but actually take this to the general public," she says. "When I looked out at the world, I just saw this pattern that I think nobody else had ever seen."

What she saw was that married people were healthier and wealthier, had better sex, and were better parents than the unmarried. "Singles must accomplish all of life's tasks by themselves," she and her co-author write. Even if they have children, couples who cohabit out of wedlock tend to behave as if their relationship were impermanent, so they avoid specialization—and lose out on the interdependence that produces many of marriage's biggest boons.

Waite starts from the stunning, and long-established, correlation between physical health and marriage for men. "Almost nine out of ten married men alive at age forty-eight would still be alive at age sixty-five," Waite and Gallagher write; "by contrast just six out of every ten never-married men alive at forty-eight would make it to retirement age." Married men are half as likely as single men—and a third as likely as divorced men—to commit suicide. Single men drink twice as much as married men the same age.

Waite then goes on to make the more contested point that both genders benefit from marriage. Because single women on average drink less and have closer friendships than single men, marriage confers much less of a behavioral health benefit on women than on men. But marriage does boost a woman's health—less by changing her habits than by giving her better financial resources and improved access to health care. Cancer-care providers have long known that having a spouse vastly improves a patient's chance of survival. Married couples also save more, have an easier time managing the parenting workload, and have a lower risk of domestic violence than cohabiting couples.

Both married men and married women seem to be less prone to clinical depression, anxiety disorders, and suicide than the single or divorced. (These benefits are not evenly distributed, however: Marriage gives more protection from alcoholism to women and more protection from depression to men.) Mental health in general seems to get a lift from a wedding, though most of marriage's psychological benefit appears to be contingent on a person considering his or her marriage very happy—currently the case with 66 percent of husbands and 62 percent of wives. Waite notes that spouses who call their marriages "pretty happy" (as about a third of husbands and wives do) are only somewhat more content than single people. Those in "not too happy" marriages—2 percent of men, 4 percent of women—are more distressed than single people. But Waite does not necessarily recommend divorce in such cases: She notes that 86 percent of unhappily married people who don't divorce are far happier five years later. (Though Waite doesn't say, no doubt some of the unhappily married who did divorce were also less miserable.)

If marriage is good for one's health, it's also not too bad for one's sex life and bank account. Cohabitants have more sex than the married, Waite reveals, but the

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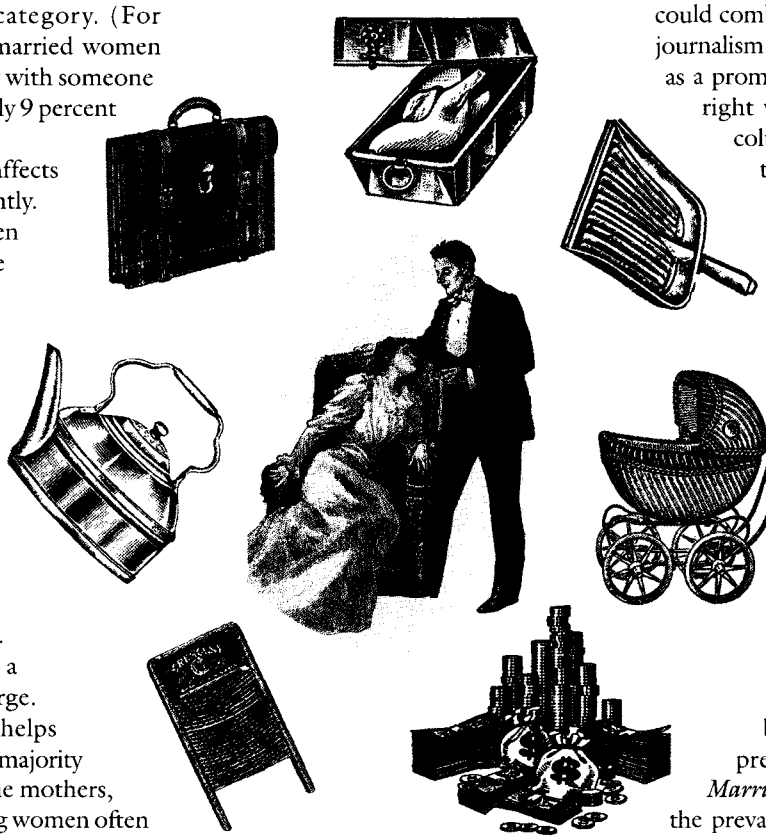
WAITE AND GALLAGHER RELY ON MATH, NOT MORALISM: THEY CLAIM MARRIAGE IS AN ECONOMIC BARGAIN YOU CAN'T AFFORD TO MISS.

married have more than non-cohabiting singles. Married men and women report more emotional satisfaction from sex than either singles or cohabitants. Waite's findings on sex exemplify the knottiness of the self-selection problem, however. People who dislike casual sex self-select into marriage, and married women seem more likely to fall into this category. (For example, 46 percent of married women say they will have sex only with someone they love—a statement only 9 percent of single men make.)

Financially, marriage affects men and women differently. Married men earn between 10 and 40 percent more than single men. The wage premium declines somewhat upon divorce, which suggests that it cannot be entirely due to self-selection. Rather, Waite and other sociologists attribute it to the support and housework wives contribute, turning their husbands into earning machines. Cohabiting men receive a premium about half as large. In this arena, marriage helps childless women. The vast majority of married women become mothers, however, and child-rearing women often cut back on paid work and earn less as a result. (At thirty-seven hours a week, a typical mother's domestic workload constitutes a virtual "second shift.") Furthermore, as Waite and Gallagher acknowledge, women often specialize in ways that put them at a skills disadvantage after divorce.

Waite and Gallagher sum up an American woman's economic options in the year 2000 matter-of-factly:

> A woman who marries and remains childless will enjoy all the emotional, sexual, and physical advantages of marriage and will be financially better off, to boot.... If she marries and has children, she will probably work less and earn less than she would if she had remained single. If the marriage lasts, she will be far more affluent, on average, than her single sisters. If the marriage fails, she assumes considerably more financial risk than her husband, but she and her children will almost certainly be better off financially than if she had had children out of wedlock.... For the overwhelming majority of women who want children, marriage today provides imperfect protection in exchange for family labor, but certainly a wedding ring still offers mothers a better financial deal than women are likely to receive without it. Inequities caused by divorce (or housework) may be a serious reason for social concern and reform, but they are not good reasons for women to avoid marriage.



TO HELP convey her controversial message, Waite picked an unorthodox fellow messenger. The object of serious loathing among fans of the sexual revolution, Maggie Gallagher is a forceful polemicist. Her path to journalism began in an unusual fashion: She got pregnant in 1982 during her senior year at Yale and chose to keep her son, whose birth she calls "a revelatory experience." Her relationship with her sophomore boyfriend fell apart, and she became a single mother. She needed work that she could combine with single parenting, and journalism fit the bill. She crafted a career as a prominent writer and editor on the right with two books, a syndicated column, and editorial positions at the *National Review* and the Manhattan Institute's *City Journal*. She now works full time from her home. (The Institute for American Values paid her salary while she collaborated with Waite.) Seven years ago, she married, and she now has another son, age five, in addition to her seventeen-year-old, Patrick.

Whereas Waite's prose is littered with references to beta columns, utility functions, and changes at the margin, Gallagher writes with bodice-ripping intensity. In her previous book, *The Abolition of Marriage* (Regnery, 1996), she decried the prevailing sex-as-fun ethos for containing none of "the deep reality of eros, of its grandeur or cruelty, ecstasy, longing, or the humiliation of losing oneself in another human being," and for containing "no hint of the terrors of loneliness, or the urgency of lust, or the need to prove one is a man or a woman, or the even more urgent desire to break through the bounds of flesh, to give oneself, to take another. No hint of passion, or love, or frailty, or longing, or hatred—no hint of the moral and emotional intensity that gives sex the power to create and to destroy." Yowza. Waite admired *The Abolition of Marriage*. Its argumentation "did no violence" to the numbers, Waite says, and she recruited Gallagher for the benefit of her lively prose.

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Waite's choice of co-author seems to correspond to a shift in her thinking about marriage. Nearly a decade ago, in *New Families, No Families,* Waite argued that only "new families" (men help with the child care and housework, women have full-throttle work lives) would dam the flood tide of "no families" (divorced parents, single mothers, childless couples, and childless singles). Waite had a different co-author then: Brown University sociologist Fran Goldscheider. "With that book, we said, 'If we don't get our act together, we'll all divorce, go off, and be by ourselves,'" recalls Goldscheider. "I definitely think it's the different sets of gender expectations driving the divorce rate."

But Waite has moved away from the thesis she and Goldscheider once shared. "She cared more about keeping the families together. I cared more about gender roles," says Goldscheider. Now Waite argues that even though men may benefit from marriage more than women do, marriage is still worth it for both partners, particularly for children's sake. *The Case for Marriage* takes for granted that a breadwinner-homemaker family is better than a "no family."

In some ways, Waite's pairing up with Gallagher resembles an intellectual remarriage. Though obviously very loyal to Waite, Goldscheider sounds vaguely like someone discussing her ex's new spouse when she says, "Maggie Gallagher and I can communicate over specific findings but not over interpretations. Her politics!" Goldscheider is thanked in the new book's acknowledgments as a true friend "who gave far more than anyone except a co-author should."

There is a tough, unyielding quality to Waite's mind—argue with her, and she will take you to the mat. She says she did not want to collaborate with another academic, partly because she wanted a collaborator who would help her break out of an academic style but also because another scholar would fight for her own personal interpretations. By contrast, Gallagher "really writes beautifully," Waite says, and "she'd never mess with me on the ideas."

WAITE'S OVERALL case sounds impressive, and there is little opposing research to contest it. You put the book down wanting to fix up all your friends. But there are weaknesses. A significant problem for Waite's argument—as so often in the social sciences—is the problem of a self-selecting sample: Does marriage make people healthier, happier, and richer, or do healthy, happy, rich people get (and stay) married more often than the sick, miserable, and poor do?

This question has been at the center of one of the thorniest and most technical debates in sociology. As New York University professor Arlene Skolnick explains, in order to prove Waite's arguments to a scientist's satisfaction, "you'd have to take one hundred men and women at random and marry half of them, while leaving the other half single." Brown University psychiatrist Peter Kramer, author of *Should You Leave?* (Scribner, 1997), notes that "this is inherently a very hard thing to study. People who are healthier to start with marry, and they can make the adjustments to make marriage successful. The 50 percent who divorce, those are not a random sample. They are not the same as the group who just doesn't consider divorce."

Waite does not deny that self-selection is at play. But she hopes to show that it doesn't account for all marriage's benefits. The expectation of permanence and mutual reliance, she believes, provides a significant portion.

Waite attempts to answer the selection objection by comparing married people with widows and widowers. If the benefits of marriage are entirely due to self-selection, widows and widowers should fare nearly as well alone as they did while married, because they should still possess the beneficial traits that enabled them to be healthy, wealthy, and happy. Yet one economist found that the widowed, like the divorced, lost assets over a five-year period, whereas married people the same age increased assets by 7 percent. Mental health data indicate that widowed people are about three times as likely to commit suicide as the married and that only 22 percent of the widowed call themselves very happy with life in general, whereas 40 percent of the married do. (Of course, Waite's critics charge that bereavement itself might account for these falloffs.)

Before Waite has even sat down at the table, she addresses her fear that she'll be pigeonholed as a paleoconservative not unlike her Chicago colleague Gary Becker, whose 1982 *Treatise on the Family* offered the classic economic analysis of marriage. "A reviewer said, 'Basically it's the Gary Becker thesis—she's arguing that women should be kept in the home,' and that's not what I'm saying at all!" she exclaims. "People tend to specialize any way they want." Both Waite and Gallagher are full-time working mothers in dual-career marriages. *The Case for Marriage* is not a brief for the two-career couple or for the woman as homemaker; it is resolutely agnostic about how work should be divided between husband and wife.

WAITE USED TO ARGUE THAT THE ROLES OF HUSBAND AND WIFE MUST CONVERGE. NOW SHE INSISTS WOMEN SHOULD TAKE ADVANTAGE OF MARRIAGE, EVEN IF MEN BENEFIT FROM IT MORE THAN WOMEN DO.

Talk to Waite in person, however, and soon enough some speculation about a "more probable" gender-based division of labor does emerge. She starts out sounding like a feminist policy analyst. Single parenting is a problem, she says, "because it shifts all the emotional and physical costs of childbearing onto the woman, which is inherently unfair. It deprives the child of the resources from the father. You can say there must be another way to extract those resources, but no one's figured out how to do it. In Sweden, they do a better job, because more of it comes from the state."

But then she pauses and begins to suggest that women and men may naturally possess different aptitudes. "The truth is," she continues, venturing into the kind of speculation an evolutionary biologist might make, "that men bond to their chil-

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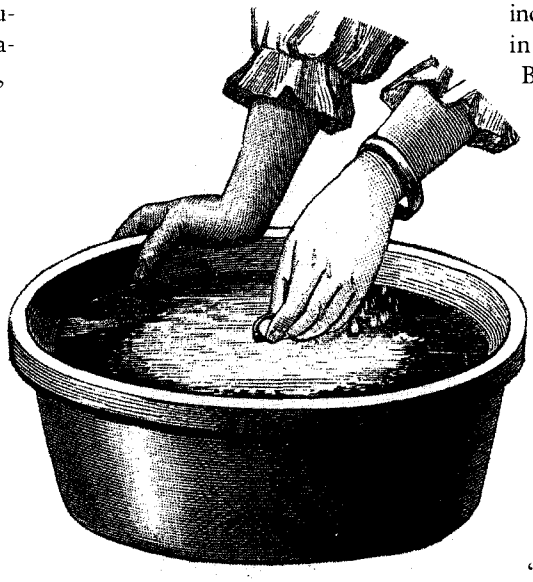
dren through the woman that they're sexually bonded to. Women bond to their children directly. The big question is how do women bond to men."

Does it make sense for both members of a couple to lead strenuous work lives? Statistics indicate that far from leading glamorous, creative careers, most married mothers are drudging away in service and clerical jobs because their families need the money. Many would prefer part-time work, but such jobs are scarce and rarely pay well. And because divorce is always a possibility, wives may stay employed to hedge their bets: It is economically dangerous to rely too heavily on a husband who might not stick around. "The argument is that where women have a reasonable expectation of a reasonable return, they're more likely to invest in their husband," says Waite. She insists that "if one person maximizes his or her career, that person's career does better, all else equal. Either you both take hits, both your careers are less, or one takes a bigger hit than the other." Of course, she says, the spouse who maximizes her career could be a woman. But Waite cites what she calls the "incredible speculations" of Stanford economist Victor Fuchs, who has argued that women tend to want children more than men do and are therefore more likely to make concessions—including career sacrifices.

"Don't you think that the women who do both [paid work and parenting] tend not to be as hard driving as the most driven men?" Waite asks. "If push comes to shove and her kid is sick—if she thinks the kid's sick, but he thinks the kid's not—she's not going to spend that extra four hours working on that legal case. Or she's not going to take that surgical residency because she doesn't want to spend eighty hours a week at work."

Gallagher is even less timid on the subject of what women want. "If you think that gender is irrelevant to the project of life, then you're as blind as a Victorian about sex," she declares. "Women want to be around their babies. It's the mammalian imperative!" That's not to say that this mammalian imperative implies male dominance. Gallagher, too, says she hopes for a time when power is shared and "equal regard" is exchanged between those who earn money and those who do nonmarket labor.

IN THE 1972 book *The Future of Marriage,* the Penn State feminist sociologist Jessie Bernard notoriously opined that marriage was good for men but housewifery was pathological for women. "To be happy in a relationship which imposes so many impediments on her, as traditional marriage does," Bernard wrote, "a woman must be slightly ill mentally." Bernard noted that isolation, rather than marriage per se, was responsible for the psychological damage.



Much has changed since Bernard's day. According to *All Our Families* (Oxford, 1998), an anthology on new family forms, about 50 percent of mothers with babies now work, and about half of two-parent families have two full-time workers. (Women, however, still earn far less than men.) The gender gap in housework is down, says Waite, not because men are doing more housework than they used to but because women are doing far less.

But even though some of Bernard's worst fears about domestic drudgery may have been assuaged, wives today initiate divorce more often than husbands do. Women file for two-thirds of divorces (men are more likely to skip legal niceties and simply desert). Waite and Gallagher argue that women are voting with their feet because work outside the home has increased their overall workload and stress level, even though it may have relieved their sense of social isolation and depression.

One of the greatest benefits marriage offers women is economic: flexibility in their work lives. In fact, quite a few of marriage's benefits to childbearing women, as Waite describes them, actually derive from a massive infusion of cash from men.

Of course, if you're an advocate of gender equity in pay, or of government-subsidized child care, this line of argument can leave you a bit queasy. "It's no surprise women are better off married than not, given the realities that their income and socioeconomic status are lower in every class!" comments sociologist Barbara Risman, author of *Gender Vertigo* (Yale, 2000) and chair of the Center for Contemporary Families, a new think tank designed as a left-leaning answer to the Institute for American Values. "There's an element of tautology here. Sure, you're better off attached to men in a sexist society organized around couples where men get all the goodies. There are two responses to this: one, that society needs to change, or two, that you better make sure women stay married."

Waite responds that this is the "posit an alternative reality" school of sociology. But her heavy emphasis on finance can make you feel as if she's trying to bribe you into marriage. It reminded me of the campaign waged by my conservative Christian grandfather. He began by denouncing my careerist priorities in a birthday card: "Remember that your reward lies not in this world!" In a Christmas card a few years later, he worried about my sexual orientation: "Don't forget that the most important decision you make will be where you spend eternity!" Finally, he decided to bargain on the terms of this world and conveyed a practical appeal via my mother: "Tell her that I'll give her $10,000 when she gets married, and then another $10,000 for the first baby." You can't help but wonder: If marriage is so great, why does Waite have to sell it so hard?

One of the ideas underpinning Waite's pro-marriage pitch is rational choice theory, the notion that people will act on their own

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best interests if they are told what those interests are. People stopped smoking and started exercising, didn't they? Don't they wear seat belts and recycle? Waite hopes that good sense will motivate people into getting and staying married. But few men lose their heads over better immune function, and few women sit anxiously by the phone waiting for news of pension processing. Seemingly irrational impulses tend to get men and women into (and out of) marriage quite effectively.

MAGGIE GALLAGHER

Indeed, it's difficult to calculate the importance of love and friendship. The feminist Shere Hite's 1987 assessment, *Women and Love*, is dubious as a work of science but invaluable as a treasure trove of female complaint: "My husband never saw me as an equal and treated me as an inferior." "I'd rather be alone. At least that way I don't have to be put down." Women today bring to their relationships a new economic leverage, but they sometimes seem to be using that leverage to obtain something fairly old-fashioned. "Let's face it," Melissa Bank, author of *The Girls' Guide to Hunting and Fishing* (Viking, 1999), recently told *Time* magazine, "you don't just want a man in your life. You only want a great man in your life."

Waite responds to this romantic impulse with outright frustration. In fact, she questions the very ideal of companionate marriage. "What people thought, and I was certainly one of those people, was that there were men out there who were extremely successful and warm and loving and always there and supportive and did a lot of housework—and they just don't exist!" she says. "If you think you're going to get your conversational and emotional needs met by your husband, forget it! It's just not going to happen, because men and women have different needs. Realize women have more need for this, and find another woman to talk to."

Gallagher dismisses the wish for a perfectly adoring and adorable mate as "ritual purity"—a craving by women for "a time when they will be utterly loved." She warns: "Don't find him unloving and uncaring because he doesn't act like a woman." And she adds that "it's unjust and not fair and not wise" to write off a husband's financial contributions or to be ungrateful for "this very nice life that this man is giving you."

Waite seems to have a warm disposition, but she also has a Lisa Simpson–like inability to empathize with romantic yearning and disappointment. "Why is Waite saying that not really satisfying lives are good enough?" asks Risman. Yet what may be most useful about Waite's analysis is that it leads you to question your rosier assumptions about happiness and satisfaction. Instead of arguing that marriage brings bliss, Waite and Gallagher show how marriage can alleviate certain forms of misery—loneliness and self-destruction for men, stressful and impoverished single motherhood for women, paternal loss for children.

On the other hand, marriages do sour, sometimes irreversibly. Divorce often triggers depression, but it would be unwise to conclude from such an observation that divorce is therefore always a mistake. Any failure at an important task in life is depressing—being fired from a job, getting kicked out of school—but failure cannot be pretended away.

WAITE AND GALLAGHER

lay much of the blame for the vulnerability of marriage at the door of unilateral, no-fault divorce law, which swept the country in the 1970s. Before its advent, even if both spouses wanted to split, one had to make a (usually fabricated) claim of adultery, cruelty, or desertion, while the other played the role of guilty party. Today, a spouse can simply walk away. Waite argues that easy divorce makes married people more skittish. They invest less in their marriage and more in themselves, and in a vicious circle, separation becomes an easy step to take.

If you were going to design a society with enduring marriages from scratch, and your options were (1) economic fetters on wives and legal fetters on husbands, or (2) economic and legal autonomy for both spouses, who would be asked to respond to this freedom with emotional maturity, the choice would be obvious. Keep women dependent, men obligated, and divorce illegal.

Says Gallagher: "There is a fair amount of evidence, in my opinion—and I don't think that this is Linda's opinion—that marriages in which women do not want to have some protection from the need for market labor are going to be more vulnerable, because they are going to rely more on husbands' ability to satisfy wives' emotional needs. Men who are more gifted at that will maintain their marriages, the less gifted will be more vulnerable to divorce."

Waite has higher hopes for two-career marriages than Gallagher does. But such marriages lead to a drastic falloff in middle-class fertility, and that alarms her. "There's not very much societal support for child rearing with all the incredible amounts of work that people do. What's going to happen? If we all work long hours and have fewer kids, in the short run we can maximize profits, but fifty years from now [the population] will all be immigrants from high-fertility countries." Indeed, if working families cannot invest in their children, succeeding generations will have economic divisions that are more and more pronounced.

So, can marriage be saved? In their final chapter, Waite and Gallagher propose a number of ways it might be: reforming the earned income tax credit so that low-income people would not be penalized by marrying; instituting waiting periods for contested unilateral divorces; limiting domestic-partnership benefits to gay couples; discouraging unmarried pregnancy; providing marriage education to high school students; reducing fees for wedding-license applicants who take a skills class; and recruiting older couples to mentor younger ones. The two writers split on the advisability of gay marriage—Waite pro, Gallagher con—though both believe there is no statistical evidence that two people of the same gender would benefit as effectively from specialization as a man and a woman would.

Will the Waite-Gallagher proposal work? The Johns Hopkins sociology professor Andrew Cherlin, author of *Marriage, Divorce, Remarriage* (Harvard, 1992), surmises that a public health campaign "might be a shrewd way to campaign for marriage now, consistent with our individualistic lens." He believes that policy changes such as tax credits for kids and waiting periods for contested divorces are "important in the symbolic sense, but they won't have much practical effect. There is a possibility for

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"IT'S UNJUST AND NOT FAIR AND NOT WISE" TO WRITE OFF A HUSBAND'S FINANCIAL CONTRIBUTION, SAYS GALLAGHER.

some minor change at the margins, but not a stunning reversal of the trends we've seen for the last 150 years." Others are even more skeptical. Diane Lye, a sociologist who recently left the University of Washington to work in the private sector, thinks calls for mentoring and marriage education are ridiculous: "It's stupid, wishy-washy, why-can't-we-all-get-along thinking, and it's unrealistic. You can wring your hands about it, but frankly it's not going to change. We've arrived at a new, stable pattern."

For all their efforts to defend marriage in terms of individual interests, it may be hard for Waite and Gallagher's ultimately communitarian ethos to penetrate our powerfully individualistic culture. Waite herself could not penetrate the consciousness of her own brother. "My brother left his wife when they had a two-year-old, a four-year-old, and a six-year-old," she says. "There wasn't any conflict. He wasn't happy! We're in e-mail contact, and he said, 'My emotional needs weren't being met.' I thought, 'Well, maybe your emotional needs don't come first here. What about your kids' emotional needs?'"

Against Waite and Gallagher's call for permanent relationships, Americans do seem especially prone to a peripatetic, distracted ethos of new enterprises, new ideas—and new relationships. The divorce rate in the United States has always been three or four times higher than that in Western European countries. Alexis de Tocqueville observed that "an American will build a house in which to pass his old age and sell it before the roof is on; he will plant a garden and rent it just as the trees are coming into bearing; he will clear a field and leave others to reap the harvest; he will take up a profession and leave it; settle in one place and soon go off elsewhere with his changing desires.... Death steps in in the end and stops him before he has grown tired of this futile pursuit of that complete felicity which always escapes him."

Who knows if this restlessness can be calmed? At a recent show in New York City, the indie-rock band Ida played a two-hundred-year-old folk song as a present to a friend on the eve of his wedding. "Are you tired of me, my darling?/Answer only with your eyes," they sang, as if the centuries-old tune might be able to inoculate a man against marital disaffection. The audience took a deep breath.

Elise Harris, a former senior editor at *Out* magazine, has written for *The New York Times Book Review*, *The Nation*, and *Civilization*. She is writing a book on the narrative of romantic love.

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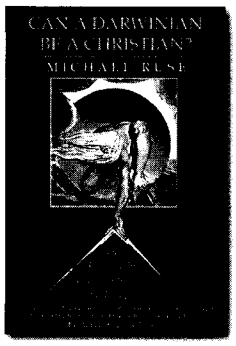
Michael Ruse



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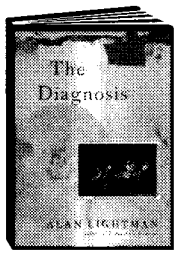

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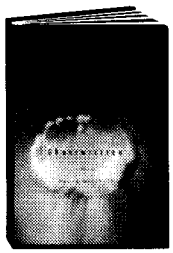

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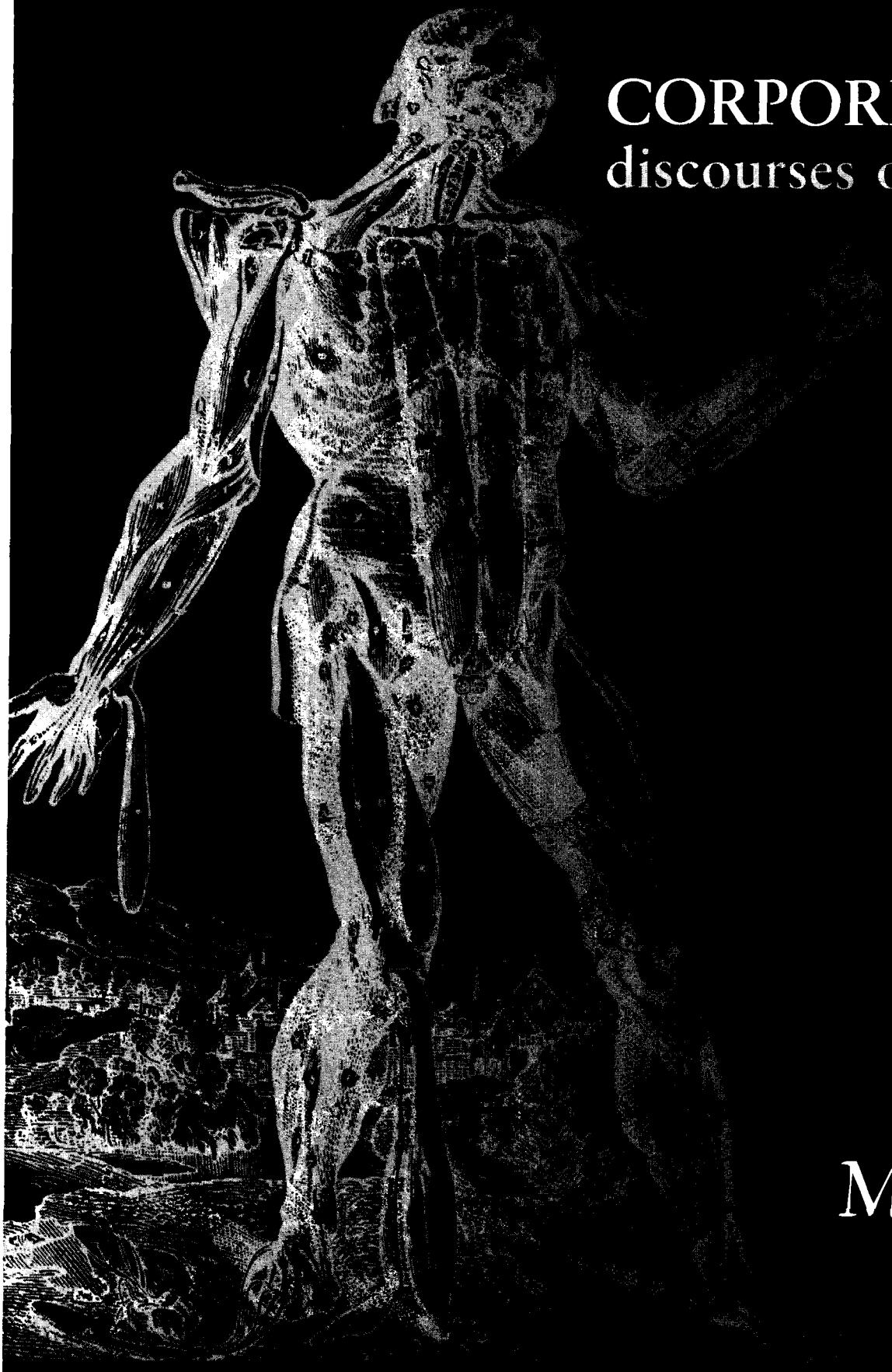

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"DEFECTS": ENGENDERING THE MODERN BODY

Helen Deutsch and
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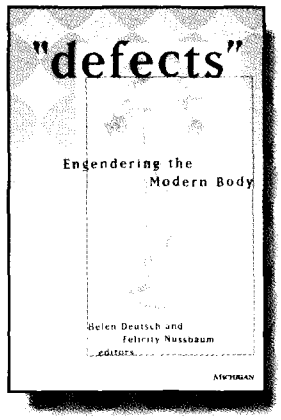

"Defects" brings together essays on the emergence of the concept of monstrosity in the eighteenth century and the ways it paralleled the emergence of notions of sexual difference. According to *Beauty's Triumph* (1758), women have been regarded since Aristotle as deformed amphibious things, "neither more or less than *Monsters*." This alliance of monstrosity with misogyny, along with the definition of sexual difference as aberration, is the starting point for this volume's investigation of monstrosity's cultural work in the eighteenth century and its simultaneous mapping and troubling of the range of differences.

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A HISTORY OF DISABILITY

Henri-Jacques Stiker
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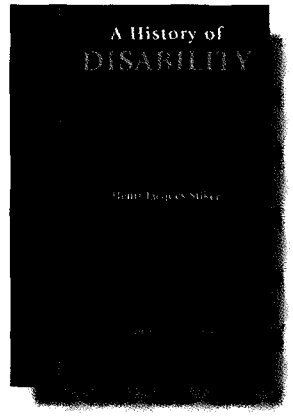

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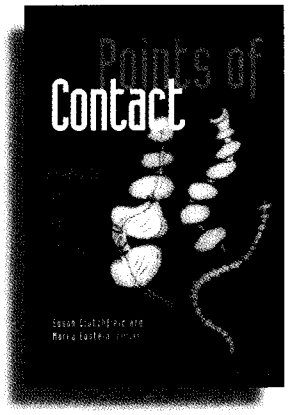
Marcy Epstein, Editors

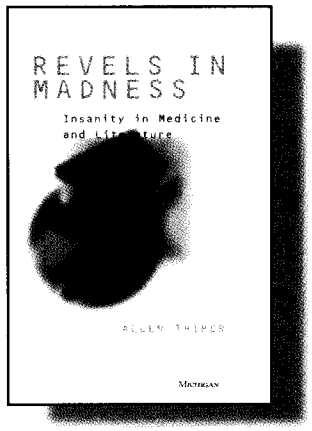


Points of Contact brings together contributions by leading writers, artists, scholars, and critics to provide a remarkably broad and consistently engaging look at the intersection of disability and the arts. The book includes essays, poetry, memoir, and fiction by a wide range of disabled and nondisabled writers, covering a broad range of subjects and concerns that lie at the intersection of disability and the arts. Topics include fetal alcohol syndrome, education, and identity; representations of disability in the visual arts and the complicated position of the disabled spectator; the similarities between beauty pageants and freak shows; disability and performance; and Helen Keller and Annie Sullivan, Franklin Roosevelt, and sado-masochistic performance artist Bob Flanagan.

6 x 9, 312 pages, 36 photographs, 2000
cloth ISBN 0-472-09711-3 ~~$47.50~~ $38.00
paper ISBN 0-472-06711-7 ~~$17.95~~ $14.36

REVELS IN MADNESS: INSANITY IN MEDICINE AND LITERATURE

Allen Thiher



Revels in Madness offers a history of western culture's shifting understandings of insanity as evidenced in its literature and influenced by medical knowledge. It traces the period from the development of Greek medicine, and of Greek tragedy and comedy, to contemporary representations of madness as shaped by modern psychiatry and psychoanalysis and portrayed in literature, surveying a remarkable range of writers, including Cervantes, Nerval, Rimbaud, Hölderin, Charcot, Freud, and Kraepelin.

Conceptions of madness in literature have reflected the cultural assumptions of the era. During medieval times, insanity was viewed as a trial sent by God, while during the nineteenth century, it was seen simply as a sign of degenerate heredity. These two points of view represent a pre-modern and a modern understanding of madness. The transition from classical theories of madness. This transition began at the end of the Enlightenment and culminates in recent women's writing that challenges the postmodern understanding of madness as a fall from language or as a dysfunction of the cybernetic system now defined as culture.

"Fascinating and important . . . a work of prodigious scholarship, covering the entire history of Western thought and treating both literary and medical discourses with subtlety and verve."

–Louis Sass, author of *Madness and Modernism*

6 x 9, 368 pages, 1999
cloth ISBN 0-472-11035-7 ~~$57.50~~ $46.00

NARRATIVE PROSTHESIS: DISABILITY AND THE DEPENDENCIES OF DISCOURSE

David T. Mitchell and
Sharon L. Snyder

Narrative Prosthesis develops a narrative theory of the pervasive use of disability as a device of characterization in literature and film. It argues that, while other marginalized identities have suffered cultural exclusion due to a dearth of images reflecting their experience, the marginality of disabled people has occurred in the midst of the perpetual circulation of images of disability in print and visual media. The book offers comparative readings of key texts in the history of disability representation, including the tin soldier and lame Oedipus, Montaigne's "infinities of forms" and Nietzsche's "higher men," the performance history of Shakespeare's *Richard III*, Melville's Captain Ahab, the small town grotesques of Sherwood Anderson's *Winesburg, Ohio* and Katherine Dunn's self-induced freaks in *Geek Love*.

"Narrative Prosthesis *is by far the most sophisticated and most wide-ranging study of 'disability and the dependencies of discourse' yet undertaken. From Shakespeare to film (not as far apart as you would think); from Montaigne to* Moby Dick *(also an interesting pair) Mitchell and Snyder postulate how we have had to imagine those human beings represented as disabled. Theoretically acute; clearly written–a must read for all of those interested in disabilities and their cultural implications."*

–Sander L. Gilman, University of Chicago

6 x 9, ca 264 pages, 2000
cloth ISBN 0-472-09748-2 ~~$49.50~~ $39.60
paper ISBN 0-472-06748-6 ~~$21.95~~ $17.56
December

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DISABLED VETERANS IN HISTORY

David A. Gerber, Editor

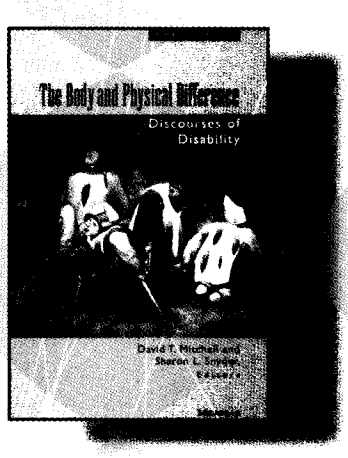
Disabled Veterans in History explores the long-neglected history of those who have sustained lasting injuries or chronic illness while serving in uniform. The contributions to this volume cover an impressive range of countries in Europe and North America as well as a wide sweep of chronology from the ancient world to the near present. The essays address the emergence of *veteran* as a political category with unique privileges and entitlements and of the care of disabled veterans as a special project—indeed one of the original projects—of the modern welfare state.

The introduction, "Finding Disabled Veterans in History," offers perhaps the first attempt at synthesizing knowledge about disabled veterans in Western societies. The three groups of essays that follow examine the representation of disabled veterans from Sophocles' Philoctetes to American feature films; the relations of disabled veterans to the state and society, as illustrated in such public policy issues as pensions, medical care, physical rehabilitation, and job retraining; and the disabled veteran's agency and experience in reentering the peacetime world. The book explores such varied topics as the place of disabled veterans in societies defeated in war; the impact of frequent changes of political regimes on the treatment of disabled veterans; the emergence of pensions and vocational rehabilitation for disabled veterans; and the abiding problem of alcohol abuse among disabled veterans.

6 x 9, ca 356 pages, illustrations, 2000
cloth ISBN 0-472-11033-0 ~~$49.50~~ $39.60
December

THE BODY AND PHYSICAL DIFFERENCE: DISCOURSES OF DISABILITY IN THE HUMANITIES

David T. Mitchell and
Sharon L. Snyder, Editors



". . . a pathbreaking, germinal work, bringing together essays that are consistently illuminating, incisive, and important. Each forces us to rethink the intellectual stranglehold of an unspoken, unacknowledged 'ableist' ideology in the humanities. . . . This work marks the coming of age of a movement that will, ultimately, mark as significant a change in our understanding of our profession—and human interaction—as the movements against the oppression of racism, homophobia, and sexism."

—Louise DeSalvo, Hunter College

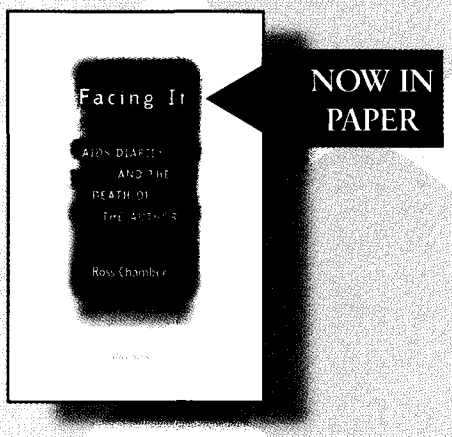
". . . Mitchell and Snyder's anthology presents the widest spectrum of 'disability studies' and will serve in the future as the benchmark against which such studies are measured."

—Sander L. Gilman, University of Chicago

6 x 9, 320 pages, 10 illustrations, 1997
cloth ISBN 0-472-09659-1 ~~$49.50~~ $39.60
paper ISBN 0-472-06659-5 ~~$18.95~~ $15.16

FACING IT: AIDS DIARIES AND THE DEATH OF THE AUTHOR

Ross Chambers



". . . provides us with a poignant analysis of AIDS autobiography in the first decade of the epidemic, one that honors the subjectivity and politics of AIDS autobiographers while avoiding victimizing those who suffer from AIDS. . . . It is literary criticism at its best."

—*Biography*

6 x 9, 160 pages, 1 illustration, 1998
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FOR YOUR EYES ONLY

THE CIA WILL LET YOU SEE CLASSIFIED DOCUMENTS— BUT AT WHAT PRICE?

BY GUS RUSSO

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AT FIRST, IT SEEMS LIKE JUST ANOTHER SCHOLARLY squabble over lingo. Then you notice the Orwellian overtones. "I have seen too many people over the years dodge the unpleasant term 'deception' by using 'perception management,'" says Central Intelligence Agency senior analyst Karl Spielmann to his academic audience. Earlier Spielmann had clarified another contested term in intelligence studies: "'Strategic,'" he explained, means "damn important!"

We are at the annual convention of the three-thousand-member International Studies Association (ISA), held in downtown Los Angeles's silo-like Westin Bonaventure Hotel. Spielmann is addressing a panel called "Denial and Deception in the Information Age." In remarks laced with Washington insider-speak—at one point he fondly refers to Henry Kissinger as "Henry the K"—Spielmann stresses the need to combat propaganda attacks waged against the United States over open information channels like the Internet. Though he says he's not recruiting academics for the CIA, Spielmann ends with a plea for concerted scholarly inquiry into methods of denial and deception (D & D). "I think we have an incipient discipline," he says.

In the question-and-answer period, a scholar asks the man from "the Campus"—as CIA headquarters in Langley, Virginia, is sometimes called—for "unclassified" examples of D & D. Before long, the panelists are discussing how governments go about misleading their enemies and how those deception techniques help spies to ferret out enemy secrets even while dissembling. As the conversation proceeds, one thing stands out. Probably it's just a coincidence: Unlike most of the thirty-odd academics in the room, Karl Spielmann is not wearing a name tag.

The "cloak-and-gown connection," as some have called it, is hardly new. At least since 1966, when *Ramparts* magazine exposed a $25 million CIA project at Michigan State University to train South Vietnamese police, intelligence-academia collaborations have stirred discomfort and disapproval. During the 1980s, revelations about the CIA's ties to academe helped spark large-scale student protests on several campuses.

Since the Cold War's end, however, the nation's universities and intelligence services have experienced a kind of détente, tied closely to the United States' new global good-guyhood. Today, university watchdogs tend to fret about corporate rather than government tugs on scholarship, and the formerly strong "CIA Off Campus" organization doesn't even have a Web site. "The opprobrium that used to attach to any relationship with government intelligence agencies has more or less vanished," notes Steven Aftergood, director of the Project on Government Secrecy at the Federation of American Scientists.

The CIA has seized this opportunity. A 1996 Directorate of Intelligence memo calls "public outreach" a top priority and targets academia in particular. According to experts on U.S. intelligence, the strategy has worked. Since the end of the Cold War, spies and scholars have grown more cozy than at any time since Vietnam drove a wedge between professors and the government. Cooperation "is now very much to the fore," says Yale political scientist and intelligence scholar Bradford Westerfield. "There's a great deal of actually open consultation, and there's a lot more semi-open, broadly acknowledged consultation." As Loch Johnson notes in *America's Secret Power: The CIA in a Democratic Society* (Oxford, 1991), cloak-and-gown relationships can take many forms. These range from occasional telephone conversations, to on-campus research arrangements and consultancies, to lecturer positions and extended scholar-in-residence programs at the CIA itself. The level of security clearance granted to the scholar depends on the sensitivity of the research and the degree of exposure to classified materials.

How close is too close? Some scholars object to all partnerships involving security clearances. The prominent University of Chicago historian and political scientist Bruce Cumings has sharply criticized ties between academia and intelligence.

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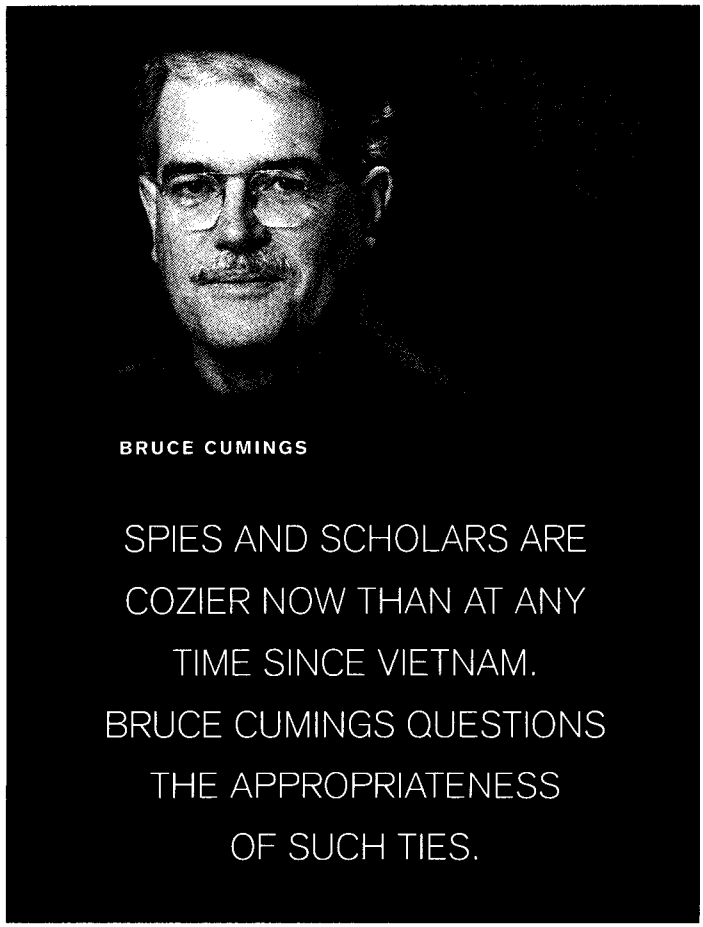
And last year, the University of Arizona political scientist David Gibbs launched a one-man campaign against what he saw as an uncritical approach to the CIA at the ISA's flagship journal, *International Studies Quarterly* (*ISQ*).

The claims of Gibbs, Cumings, and others prompt reactions ranging from bafflement to charges of conspiracy-mongering from other international studies professors, some with and some without CIA ties. Have U.S. scholars of international affairs grown too close to the intelligence community? Or has the Cold War's end made security clearances acceptable in academe, despite the objections of some now-old New Leftists who are still instinctively storming the Pentagon?

"Research and analysis are at the core of intelligence," writes the Yale historian Robin Winks in *Cloak and Gown: Scholars in the Secret War, 1939–1961* (William Morrow, 1987). For this reason, CIA analysts like Karl Spielmann want scholarly aid in solving intelligence problems. When academics work with the CIA, at least in theory both parties stand to gain: The academics get access to the best information, though it's classified and available for their eyes only; meanwhile, the agency gets access to the best brains.

As Winks details, the Cold War partnership between academia and intelligence originated with the World War II Office of Strategic Services, or OSS—the agency some called "Oh So Secret"—whose research and analysis (R & A) branch brought together the nation's top minds to outsmart Adolf Hitler. Throughout most of the war, R & A was led by a Harvard diplomatic historian, William Langer. The OSS later morphed into the CIA, which pursued the "hot war" model of cloak and gown into chillier international climes and cultivated its connections at elite universities to great effect. ("Ph.D. intelligence" was J. Edgar Hoover's term of derision for the agency.) As the late Columbia historian Sigmund Diamond documented in *Compromised Campus: The Collaboration of Universities With the Intelligence Community, 1945–1955* (Oxford, 1992), Harvard's Russian Research Center, established in 1947, was modeled on the OSS's Soviet division and had close covert ties to the CIA from its inception. "The intelligence aspect of the work of the Russian Research Center and the research aspect...cannot be distinguished," Diamond concluded.



BRUCE CUMINGS

SPIES AND SCHOLARS ARE COZIER NOW THAN AT ANY TIME SINCE VIETNAM. BRUCE CUMINGS QUESTIONS THE APPROPRIATENESS OF SUCH TIES.

Compromised Campus, the product of Diamond's lengthy bureaucratic battle for classified documents under the Freedom of Information Act, serves as a kind of urtext for scholars who find something sinister in the early history of university-intelligence relationships. (It also claims that William F. Buckley and Henry Kissinger spied on their colleagues for the FBI while at Yale and Harvard, respectively.) Diamond's work is admiringly cited in *Universities and Empire: Money and Politics in the Social Sciences During the Cold War* (New Press, 1998), a collection edited by American University communications professor Christopher Simpson. In his introduction to the volume, Simpson unveils an astonishing statistic: U.S. military and intelligence agencies, working closely with leading foundations (Ford, Carnegie, and Rockefeller), provided the largest single source of funding for major social scientific research in the 1950s. One of the earliest international affairs institutes, MIT's Center for International Studies (CENIS), grew out of the State Department's psychological warfare initiative, Project Troy, and was clandestinely underwritten by the CIA during the early 1950s. A number of other academic institutes were predominantly, and often covertly, funded through such channels, including Princeton's Institute for International Social Research and Columbia's Bureau of Applied Social Research.

After the 1967 exposure of CIA ties to the National Students Association, Lyndon Johnson asked Undersecretary of State Nicholas Katzenbach to chair a commission on the CIA's relations with academe and private voluntary organizations. The commission's conclusion was stark: "It should be the policy of the United States government that no federal agency shall provide any covert financial assistance or support, direct or indirect, to any of the nation's educational or private voluntary organizations." The agency quickly canceled many of its contracts with professors. But as later events proved, at least some undisclosed funding remained in place. In the mid-1980s, the *Harvard Crimson* made national news by asserting that the CIA had provided funding to international relations professors Richard Betts and Samuel Huntington and to Middle East expert Nadav Safran. Safran used CIA money to help pay for an academic conference without notifying attendees. Harvard censured Safran in 1985.

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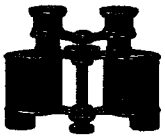

In the wake of the Safran affair, CIA deputy director Robert Gates gave a speech at Harvard in which he defended the CIA's relationships with scholars. At the same time, though, he tried to defuse some of the controversy. If a university had explicit rules against allowing faculty to conduct classified research, Gates said, the agency would abide by those rules. In the case of CIA-sponsored conferences, the agency would encourage organizers to inform attendees of the funding. Most significantly perhaps, Gates said the CIA would allow researchers to disclose that they had worked for the agency, unless the agency determined that "formal, public association of CIA with a specific topic or subject would prove damaging to the United States."

Among the most controversial CIA policies is its insistence that scholars sign a lifetime secrecy agreement before receiving a security clearance. According to CIA spokesman Tom Crispell, this means they must submit for review any books or articles they write that touch on the topic of their classified access or the broader subject of intelligence. Crispell stresses that the review is not editorial; rather, it is designed to prevent inadvertent disclosure of classified materials. But some allege that the review process can be highly politicized. And as the recent cases of Wen Ho Lee and John Deutch show, academics, like anyone else, are legally liable if they act improperly with classified information.

Chicago's Bruce Cumings is adamant that security clearances are simply incompatible with the obligation of scholars to "speak truth to power," as international relations guru and Vietnam protester Hans Morgenthau once put it. Says Cumings, "Professors involved in international affairs should not have security clearances from any governments, including their own." If academics want to heed Karl Spielmann's call to research "denial and deception," he says, "why don't they go join the CIA and stay there, and stop pretending they're professors who subscribe to canons of truth, objectivity, and honesty in the classroom?"

A frequent writer on east Asia for *The Nation*, Cumings is best known for a two-volume history of the Korean War, but he has also taught in two political science departments. In 1997, he started a small war in the *Bulletin of Concerned Asian Scholars* (*BCAS*) with an article titled "Boundary Displacement: Area Studies and International Studies During and After the Cold War" (an edited version appears in *Universities and Empire*). The article traces the development of the two closely intertwined fields of area studies and international studies at a time when universities—flush with foundation, intelligence, and military research subsidies—were convulsed with McCarthyite campus purges. Of the early history of area studies, Cumings writes, "It is only a bit of an exaggeration to say that for those scholars studying potential enemy countries, either they consulted with the government or they risked being investigated by the FBI." And in the 1990s, he suggests, things may not have changed *that* much: The 1991 National Security Education Act, since gutted by the Gingrich budget cut, created federal area and language studies programs run out of the Defense Intelligence College on the Pentagon's dime. Fellows studying abroad were required to "make a good faith effort" to find employment in national security.

In Cumings's view, far too many scholars today, particularly in international relations, collaborate with the government. "It's quite common for people in the IR field, younger and older ones, to go to the National Security Council for a while or to the CIA as consultants," he says. Cumings believes this both creates intolerable burdens on academic openness and skews scholarship: "That's one of the reasons the subdiscipline of international relations is so conservative and so concerned with realpolitik and realism as the major paradigms of inquiry, which sort of fits with what national security managers believe in."

Perhaps with this concern in mind, Cumings closed the *BCAS* version of "Boundary Displacement" with this exhortation: "Abolish the CIA, and get the intelligence and military agencies out of free academic inquiry."

Fighting words like these triggered a symposium response titled "Round Up the Usual Suspects: Cumings's Misdirected Search for Post–Cold War Enemies of Academic Independence." Indeed, it's no struggle to find views sharply at variance with Cumings's. Referring to the 1950s sub-rosa funding of international studies institutes, Bradford Westerfield says, "I'm not one of those people who get outraged about ancient history unless I think it is reflected in current patterns of behavior." And whereas Cumings would argue that only in "conditions of total war," such as during World War II, should scholars work extensively with the military and intelligence, Westerfield opines that CIA funding of CENIS and other institutes wasn't wrong to begin with—or not unless "the Cold War itself was just an American chimera that could...have been wholly avoided if we'd just caved in to permanent Communist domination over Eastern Europe and over the left in most of western Europe and Japan."

In general, academics who have done classified work strenuously protest that their scholarship and teaching remain

"THE AGENCY HAS LARGELY REFORMED ITSELF," WESTERFIELD SAYS, BUT THOSE REFORMS "ARE ETERNALLY SUSPECT IN THE EYES OF A VERY EMBITTERED GENERATION OF SCHOLARS."

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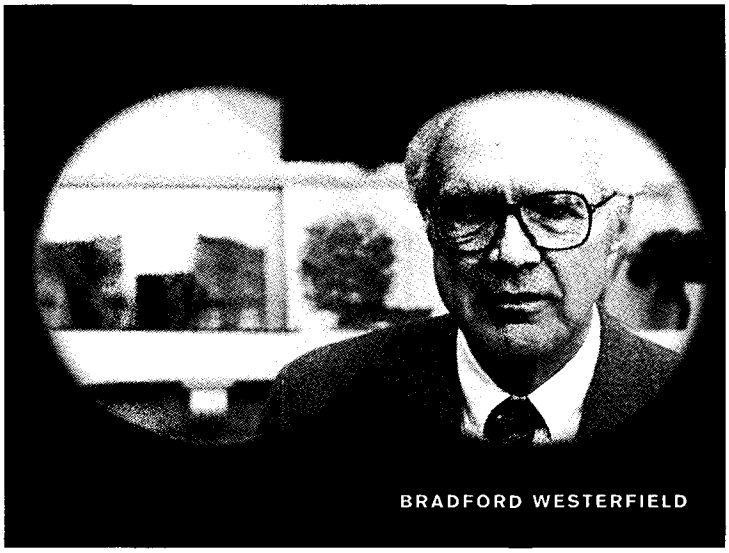
untainted. For Harvard Kennedy School dean Joseph Nye, who chaired the CIA's National Intelligence Council—and has held positions in the State Department, Defense Department, and National Security Council—classified consulting is acceptable, provided it's not secret. Rather than demonizing academic consulting, Nye says, "I think the taboo should be against doing classified work in a university setting," noting that Harvard has disallowed such practices. But as far as charges of conflict of interest are concerned, Nye insists that his intelligence ties have not prejudiced his scholarship. "I certainly have not tried to write things which are for the sake of the government," he says.

In fact, some scholars say their classified work has made them more critical of the government rather than less so. Columbia's Robert Jervis, currently president of the American Political Science Association (APSA), has consulted with the CIA on numerous occasions, including work on a 1978 "postmortem" on Iran after the CIA was embarrassingly caught unawares by the mounting movement to topple the U.S.-backed Shah (the revolution took place a year later, in 1979). Though he admits it was probably unusual back then because of CIA regulations, Jervis says he was allowed to tell his colleagues what he was up to: "I told my one left-wing graduate student that if he wanted to change advisers, he should be free to do so. He laughed and said, 'I could care less.'" In an e-mail, Jervis describes how his insider experiences proved disillusioning: "In the fall of 1978, I took advantage of the fact that I had clearances to read the Top Secret rationales for the MX [missile] program. Previously I had been skeptical, but had believed that the government had information and analyses that made the program plausible if not compelling. In fact, I found the justifications were very flimsy." Jervis says this experience ultimately led him to write two books highly critical of U.S. nuclear strategy—and he suspects these books prevented him from getting other consultancies.

But if Jervis is right in asserting that government work doesn't blunt one's critical edge, then what's all the fuss about? Jervis chalks up much of the concern over cloak and gown today to "paranoia." Those who claim that academics face censorious pressures have it precisely backward, he says. "People follow their politics, and that takes them into government, not vice versa."

Some academics see government ties as producing not servile scholarship but better-informed foreign policy. Academic consultancies can help prevent intelligence errors resulting from inadequate analysis, says Daniel Deudney, a Johns Hopkins political scientist who has consulted for numerous government security and intelligence agencies, including the CIA. "The reluctance of academics to talk to the CIA is against everyone's interests," Deudney avers. Robert Keohane, former president of the APSA and of the ISA, has chosen not to be bound by ties to the government, but he believes that scholars who choose otherwise are rewarded with considerable influence. "I think there are trade-offs in life," says Keohane. "I don't have any day-to-day influence over policy. I can't pick up the phone and call the secretary of state or defense because they're personal friends or I work for them. Right? So I trade that off, and for that I get my independence and my ability to theorize without ever worrying about what they think of me."



BRADFORD WESTERFIELD

Are the trade-offs really that cut-and-dried? Cumings, for one, is not convinced that academic assistance promotes a more responsive foreign policy establishment. Citing the United States' history of crises with North Korea—a track record that has only recently improved under the Clinton administration—Cumings observes that any number of readily available sources indicated the flaws in the government's policy. "And nonetheless the government continued doing what it had been doing," he says. "It wasn't for a lack of analysis, or getting better minds in government: It was fundamentally because of our partisan politics."

Politics is, after all, at the heart of the matter—and there are few topics more politically charged than the history and reputation of the CIA. Although the CIA draws only about $3 billion per year from the United States' roughly $30 billion dollar "black budget" for intelligence, it's usually the first agency that comes to mind when one thinks of cloak and dagger—or cloak and gown. The high-tech Pentagon intelligence agencies—the National Security Agency, Defense Intelligence Agency, and others—collectively receive almost ten times as much funding as the CIA per year. But whereas the Pentagon agencies are generally considered more politically conservative and military in character, the CIA was a favorite tool of liberal cold-warrior John F. Kennedy, and its founder, Allen Dulles, held scholars and intellectuals in high regard. Frances Stonor Saunders's recent book *The Cultural Cold War* (New Press, 2000) documents the agency's secretive efforts to promote the liberal anticommunism of the *Partisan Review* and other highbrow publications in the 1950s and 1960s.

The respect between scholars and spies has not always been mutual. During the Cold War, the CIA earned an unsavory reputation in academe thanks to controversial covert operations that ranged from staging a coup in Iran in 1953, to attempting to thwart the election of Salvador Allende in Chile in 1970, to plotting to assassinate Fidel Castro and other world leaders. During

MICHAEL MARSLAND/YALE UNIVERSITY

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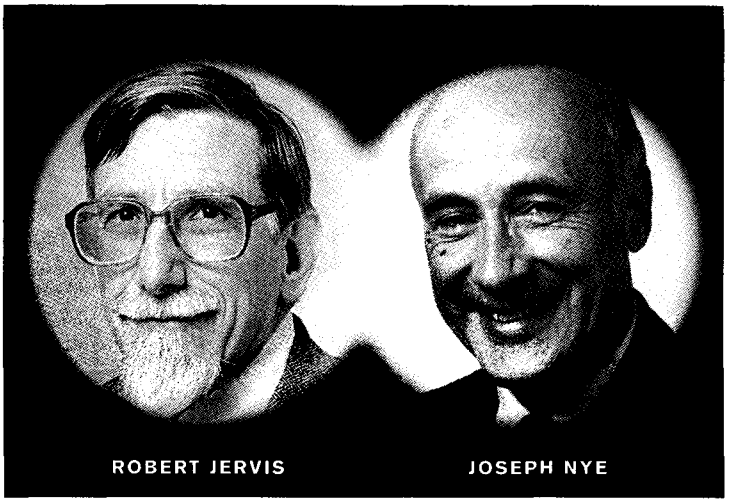
the 1980s, the agency provided funding, training, and equipment to Nicaraguan contras attempting to overthrow the Sandinista government—a policy that ultimately exploded in the Iran-contra scandal.

Some scholars argue that the CIA has cleaned up its act and that today's critics assail the agency more out of reflex than substantive grievance. In general, the CIA's bad press has begun to peter out, perhaps because the agency, by and large, hasn't been involved in the same types of controversial covert operations that characterized the Reagan era. According to Harvard's Joseph Nye, the current international climate has much to do with this as well. People are realizing, he says, that "if you're going to deal with some of the new threats that we face, such as terrorism and mass destruction, in fact you're going to need a CIA."


ROBERT JERVIS JOSEPH NYE

Bradford Westerfield stresses that the CIA is no longer—if it ever was—the "rogue elephant rampaging out of control" suggested by Senator Frank Church during the Watergate era, when Senate hearings first uncovered many of its most shocking activities. "The agency has largely reformed itself on the one hand," Westerfield says, but "those sets of reforms are eternally suspect in the eyes of a very embittered generation of scholars whose formative years were the late 1960s or early 1970s. That gulf is probably unbridgeable."

The CIA continues to relive its past partly due to the slow pace of declassification. Westerfield notes that with each new discovery about the Cold War era, a new wave of resentment crests in academe. (Most recently, declassified documents have definitively linked the CIA to Chile's brutal former chief of secret police.) "This could go on for ten or twenty years," says Westerfield. Robert Jervis currently chairs a panel of scholars that advises the CIA on declassification. Even as such material trickles into circulation, some scholars worry that progress is at best desultory. George Herring, a University of Kentucky historian who was rotated off the declassification panel, once remarked that many historically significant documents will remain permanently classified "if the people in the intelligence agencies have their way."

An old joke derides the International Studies Association as "white guys with ties / talking about missile size." Judging from this year's conference, the characterization hardly seems accurate. Intelligence and security studies, after all, are just two strands of interest among the association's twenty sections. The conference includes panels on peace studies and the global environment and features papers with titles like "How Queer Are International Affairs?" Though ISA membership is two-thirds North American, accents abound at the conference; for every CIA analyst lecturing, it seems, there's a woman in a sari.

Still, during a night of table-to-table at the Westin Bonaventure's alcohol-licensed coffee bar, I encounter two conference attendees with intelligence ties. One of them, Enrique Gallego, seems the perfect embodiment of Cumings's claim that many in international relations pursue career trajectories that involve classified work. A Ph.D. candidate in international relations at the University of Chicago at Illinois, Gallego is tall and heavyset with a buzz cut. His dissertation will examine the modernization of the Chinese military and its effect on U.S. foreign policy. As an army officer, Gallego tells me, he has done some military intelligence work and has security clearances. As for the CIA, Gallego simply says it's never cut him a check.

Gallego doesn't believe scholars should necessarily divulge their CIA connections; he thinks it can damage one's reputation. "Once you're tagged as an intelligence worker, you're tagged," he explains. Gallego has plenty of criticism for what he terms the agency's "cowboy era": "In the old days, the CIA would get a little full of itself, and it would do some bonehead things." But though "they've had their parties," today's CIA, according to Gallego, has reformed.

But even if the CIA's Reagan-era "parties" are over, critics like David Gibbs contend that such exploits have left a troubling legacy—one that cloak-and-gown connections prevent international studies scholars from investigating.

Gibbs first got his back up when he read an article by Robert Snyder of Southwestern University titled "The U.S. and Third World Revolutionary States: Understanding the Breakdown in Relations," published in the June 1999 issue of *International Studies Quarterly*. Snyder argued that the Cold War tension between the United States and three revolutionary states—Cuba, Nicaragua, and Iran—was provoked by the revolutionary states. The United States, after a period of hesitation, then reacted aggressively. In a seven-page letter to the editorial board of *ISQ*, Gibbs and three other scholars objected that Snyder's article contains "extensive and systematic distortions of evidence, and omits vitally important information that runs contrary to its thesis"—namely, that the CIA was meddling in the internal politics of each of Snyder's case-study countries.

Gibbs and the others suggested that *ISQ* run their letter as a rejoinder. But *ISQ* does not print letters; the journal's policy is that all articles must go through peer review. The editors invited the authors to

FROM LEFT: COLUMBIA UNIVERSITY; © MARTHA STEWART

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THE CIA MAY NO LONGER BE A "ROGUE ELEPHANT RAMPAGING OUT OF CONTROL." BUT DO ITS CURRENT ACADEMIC TIES PREJUDICE SCHOLARSHIP ABOUT THE COLD WAR ERA?

submit "a proposed response, which we would distribute to anonymous reviewers." But instead of submitting a paper to *ISQ*, Gibbs spilled his concerns to the *Chronicle of Higher Education*, which ran a story in September 1999. Gibbs then wrote back to *ISQ* editor Richard Mansbach, saying, "The fact that *ISQ* was willing to publish an article as problematic as Snyder's must raise questions regarding the selection and competence of referees. Also, there has been a regrettable association of key international relations scholars with various government agencies, and some of these may pose problems for scholarly objectivity."

In an interview with *Lingua Franca*, Gibbs limned more fully what he had in mind with that last sentence. He pointed out that Mansbach, currently at Iowa State University, was involved in a 1984 scandal at Rutgers University. Mansbach and another professor had conducted a class on foreign policy in which student papers would be submitted as part of a research project for the CIA. The professors had not had their project properly "endorsed" and "administered" by the university, the *New York Times* reported, and had not adequately informed the students that they would be participating in such a project. As a result, the university reprimanded them for having "acted inappropriately." Gibbs comments: "One of the specific points at issue here was misdeeds by the Central Intelligence Agency, and so I was just uncomfortable having an editor with those kinds of connections acting to supervise the review of any critique I would have made."

But according to Mansbach—who says the Rutgers incident is "not germane" to the controversy—more transpired before Gibbs went to the *Chronicle*. Mansbach says he explicitly offered to recuse himself from the *ISQ* editorial process in the review of Gibbs's piece and to send it to another review team. "If he thinks one of the editors is a problem...when that editor steps out, what more do you want?" asks Mansbach. Gibbs denies ever having received such an offer, although another *ISQ* editor, Patrick James, also at Iowa State, confirms that it was made. When *Lingua Franca* asked Mansbach to provide documentation of the offer, he refused.

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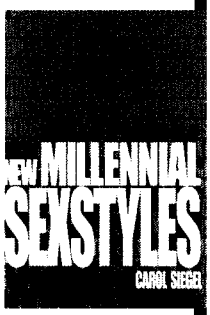

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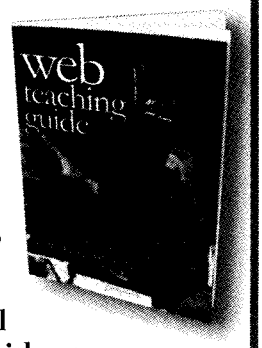

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The responses to Gibbs's campaign have been varied. A number of scholars agree with his basic criticisms of Snyder's article but disapprove of his shirking peer review and appearing to judge Mansbach on the basis of events that took place in 1984. William Robinson, a sociologist at New Mexico State University who signed Gibbs's original letter to *ISQ*, calls Snyder's article a piece of "ideology disguised as scholarship" but says *ISQ*'s offer was satisfactory to him. When Gibbs decided not to accept the offer, Robinson ceased to have any involvement with the issue. Similarly, Craig Murphy of Wellesley College, current ISA president and editor of the journal *Global Governance*, says he wishes Gibbs had submitted his rejoinder for peer review: "I think he has a number of significant points."

From Snyder's perspective, ever since Gibbs has started talking, everyone has ignored his side of the story. Snyder emphasizes that, the details of his case studies notwithstanding, he was attempting to make a theoretical point about the internal politics of revolutionary states. He also objects to the way he's been painted: "You read the *Chronicle* article, and you'd think that I was some right-wing yahoo from Texas." Snyder says he has never worked for the government, plans to vote for Gore, and comes from Pennsylvania. He also points out that a distinguished review team accepted his article, and that quite a number of leading scholars in international relations have praised the piece. One, the Tufts political scientist Tony Smith, wrote to Snyder in a letter, "Congratulations on being a *succès de scandale*. But of course the scandal is the arguments of your critics."

"THE KIND OF CLEARANCE MOST OF US GET DOESN'T GIVE US ENOUGH ACCESS," SAYS THOMAS VOLGY. "BUT IT GIVES US GREAT STORIES."

Snyder nearly matches Gibbs when it comes to provocateurship, saying the controversy stems from Gibbs's politics. He alleges: "I think, from a sociological point of view, what Gibbs represents is Marxism on the defensive. I think Gibbs feels that if Marxists can't claim to explain U.S. foreign policy toward Third World radical states, then what can they explain?"

Gibbs says whether or not he's a Marxist is irrelevant to the arguments he's trying to make. As for going outside the peer review process? "I have no regrets."

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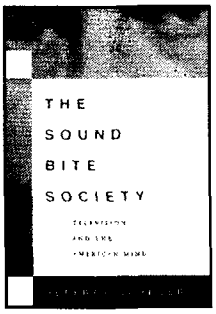

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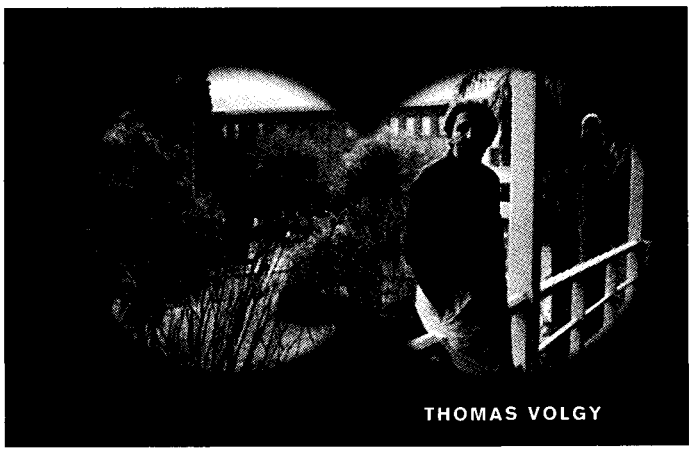

THOMAS VOLGY

UNIVERSITY OF ARIZONA

If academics do sacrifice some of their independence when they work for the CIA, what do they get in return? Not very much, suggests one of Gibbs's colleagues at the University of Arizona, Thomas Volgy.

When I meet Volgy at the conference, he looks harried, if not harassed. As the executive director of the ISA, he's bogged down by a variety of administrative tasks—during our conversation, his walkie-talkie keeps going off. Still, he devotes a considerable amount of time to talking about Gibbs's claims and scholarly connections with the government. Finally, Volgy describes his own insider experience: two stints at the State Department on a scholar-diplomat exchange program.

"I'll tell you, the kind of clearance most of us get doesn't give us enough access, but it gives us great stories," Volgy begins. "And I tell my story in my class all the time. I walked past an office called the 'Office of Weather and Climate Modification.' And I knocked on the door, I walked in, and there was this guy sitting behind the desk, but there was nothing on the desk. And I said, 'What do you do?' and he said, 'I can't tell you.' And I said, 'I've got security clearance,' and he said, 'Not for me, you don't.'" Volgy laughs.

"For months, I had these nightmares about what this guy was doing, right? And it gives me this great set of stories," he says. "If I could penetrate in there, and then I couldn't write about it, and I couldn't talk about it, what the hell was I doing there in the first place? Those are my two responsibilities as an academic: Write about it, talk about it. So if you take that away from me, I cease to be an academic. Then I may be somebody who becomes a consultant, but not an academic."

For a second, Thomas Volgy sounds a lot like Bruce Cumings or David Gibbs. But then he slows down, becoming again the soft-spoken, walkie-talkie-wielding administrator. "Yeah, you bet there's a problem there," he says. "How big the problem is, it's really hard to tell. Most of us don't get that kind of clearance. You know, we get to see the desk. And the smile of polite refusal."

Chris Mooney is a writing fellow at the *American Prospect*. His article "Showdown: The Scholarly Fight Over Guns in America" appeared in the February 2000 *LF*.

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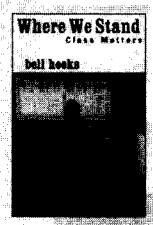

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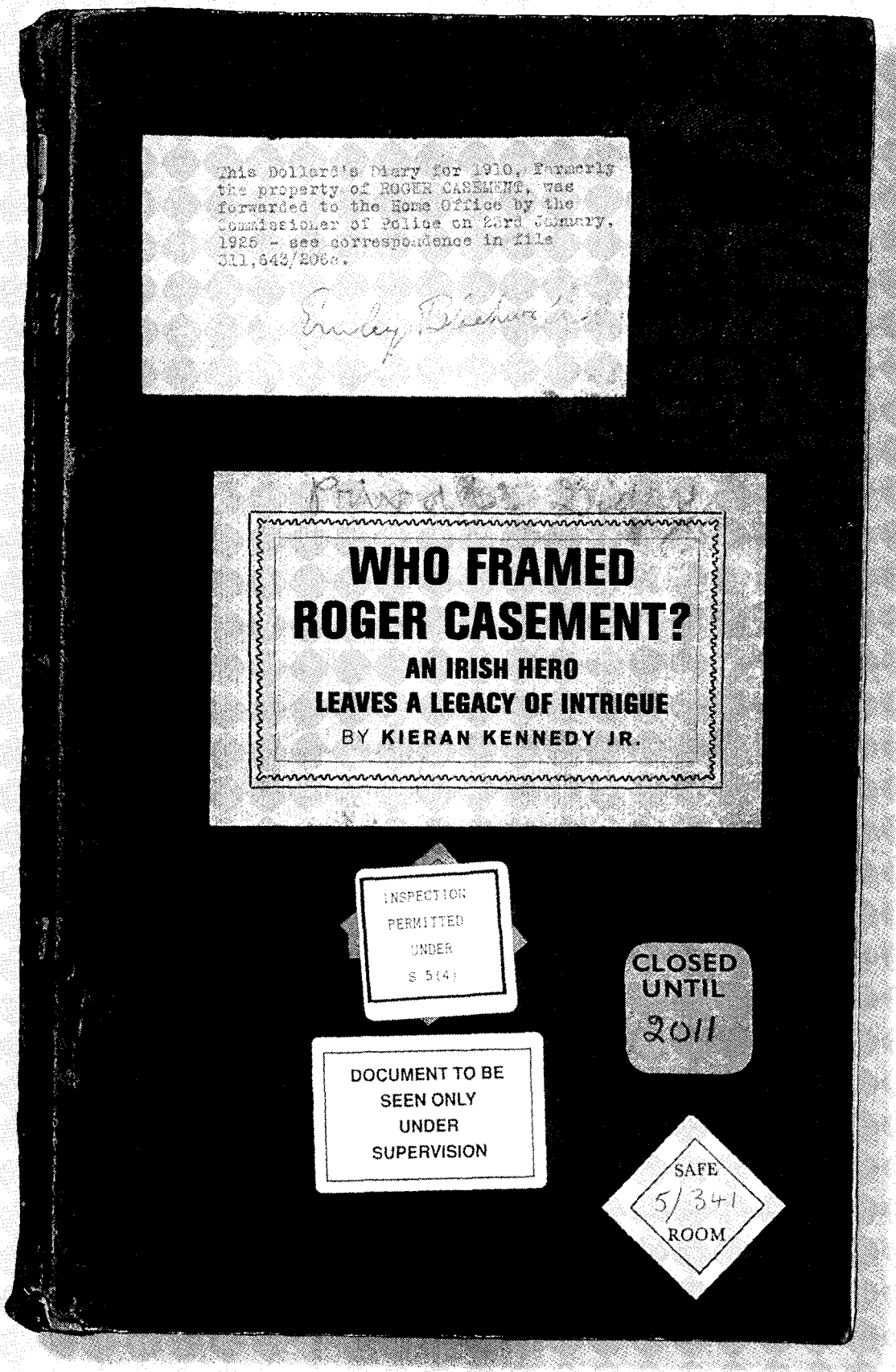
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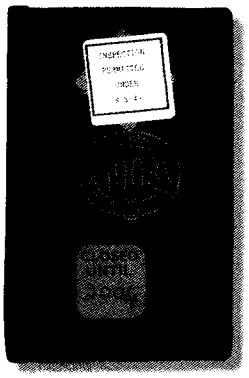
IN JULY 1916, THE BRITISH GOVERNMENT leaked the news that Sir Roger Casement, a former diplomat knighted for his humanitarian work in Africa and South America, kept a diary recording his sexual encounters with men. A sample entry, dated June 25, 1911: "Lovely Italian Ice boy, 18. Tall, splendid loved, also young dark boy, *huge* wanted awfully. To Best Station and there darkie by Watt's Grove. Orotaru 2/-. Lovely boy wanted too."

Certainly the timing of the revelation was suspicious. In April 1916, Casement had been arrested for gunrunning on behalf of Irish revolutionaries. Irish nationalists suspected that the British had fabricated the diaries to undermine clemency appeals on Casement's behalf. On August 3, 1916, the British hanged Casement for treason, and for decades afterward, popular opinion in Ireland regarded the diaries as forgeries. After an initial showing to a highly select group, the government kept the diaries in almost total secrecy in Scotland Yard, a decision that did nothing to alleviate public suspicion.



Opinion began to shift in 1959, when the Paris-based Olympia Press obtained rough transcripts of several diaries and published them as *The Black Diaries: An Account of Roger Casement's Life and Times.* To many, the sheer amount of detail suggested authenticity. In their biographies of Casement published in the 1970s, Brian Inglis and B.L. Reid accepted the texts as genuine. After the publication of Roger Sawyer's *Casement: The Flawed Hero* in 1984, the authenticity of the diaries became scholarly consensus.

In 1994, the British government finally agreed to release the diaries. Soon after, two scholars, Roger Sawyer and Angus Mitchell, began to prepare an authoritative edition for Pimlico Press. Midway through the project, however, Mitchell decided that the sex diaries had been forged after all. He and Sawyer went separate ways: Mitchell published *The Amazon Journal of Roger Casement* (1997), and Sawyer produced *Roger Casement's Diaries, 1910: The Black and the White* (1997).

In the wake of these competing editions, Casement has received renewed attention, particularly in Ireland. The scandal surrounding his death occupies a place in Ireland's national psyche similar to that of the Dreyfus Affair in France's. But whereas Jewishness preoccupied French politicians, artists, and intellectuals, it is the specter of homosexuality that concerns the Irish. "The Casement controversy is the mother of all Irish controversies," says Villanova professor Lucy McDiarmid, author of the forthcoming *Controversy: Episodes in the Irish Culture Wars.* "It's not just about sexuality, but about homosexuality; not just about nationalism, but about gunrunning; and not just about a possibly gay, possibly straight patriot, but about a half-Catholic, half-Protestant one. There's a Casement for everyone."

Including the highest politicians in Ireland. In April 1999, at an event commemorating the 1916 Easter Rising, Bertie Ahern, the Irish taoiseach (pronounced "T-shock," the title is roughly equivalent to prime minister), reopened the question of the diaries' authenticity. "I think in justice to the memory of Sir Roger Casement, there is now a compelling prima facie case for a new and rigorous inquiry into the provenance and genuineness of the so-called black diaries," he announced.

Responding to Ahern's call, the Royal Irish Academy (RIA) held an invitation-only conference in Dublin in May 2000. Scholars of literature, history, anthropology, and human rights from Britain, Ireland, Continental Europe, and the United States convened to discuss Casement. The man's sexuality was not on the agenda, but the authenticity of the black diaries was. And it was impossible to investigate the question of authenticity without stirring up other questions: Were the Irish politicians ready to accept a gay nationalist hero? And was it plausible that Casement was both a humanitarian and a john?

ROGER CASEMENT was born in county Dublin on September 1, 1864. His family hailed from county Antrim, in the north of Ireland, where he was raised by his uncle after his parents died. He was educated at a Church of Ireland school and raised as a Protestant, in the tradition of his father's family. His mother, however, may have had him baptized as a Roman Catholic. On the day of his execution, in what was perhaps a dying gesture of Irish nationalist solidarity, he took his first Holy Communion in the Catholic Church.

Casement spent ten years in Africa as a surveyor and construction supervisor before he joined the British Consular Service in 1895. He worked his way up the service's hierarchy and was appointed consul to the Congo in 1898. He later served as consul in Brazil, first in Santos and then in Rio de Janeiro, where in 1908 he was promoted to consul general. In 1911, King George V knighted Casement for his exposure of violent exploitation in Leopold II's Congo and for his reporting of the atrocities that capitalist speculators in the Amazon River basin had committed against indigenous peoples. In the last decade of his life, Casement became more and more involved with the Irish nationalist cause. He resigned from the Consular Service in 1913, partly because of ill health. From then on, he devoted himself full-time to the grievances of the Irish, whom he dubbed "the White Indians of Connemara."

During World War I, Casement served as the Irish independence movement's link to Germany. He tried to persuade Irish prisoners of war to desert the British army and fight alongside the Germans, in the hope that a German victory might lead to Irish independence. Only fifty-two men joined him. In fact, some of the soldiers he spoke to hurled mud at him, and many of those he did manage to enlist were, as his biographer Roger Sawyer explains, "a pretty drunken crowd who were trying to get out of prison camp into somewhere else where they could have more fun."

Furthermore, as Casement soon realized to his chagrin, the Germans were not seriously interested in an Irish independence movement. In April 1916, Casement left Germany on a gun-smuggling mission designed to arm an uprising scheduled for Easter. He was arrested on the beach at Banna Strand. (Some believe he was rushing home to persuade his fellow leaders that the planned rebellion was premature.) In any case, the 1916 Easter Rising went ahead, and, although it was defeated, the Proclamation of the Republic read by Patrick Pearse at the General Post Office in Dublin became a talismanic document of the Irish Free State.

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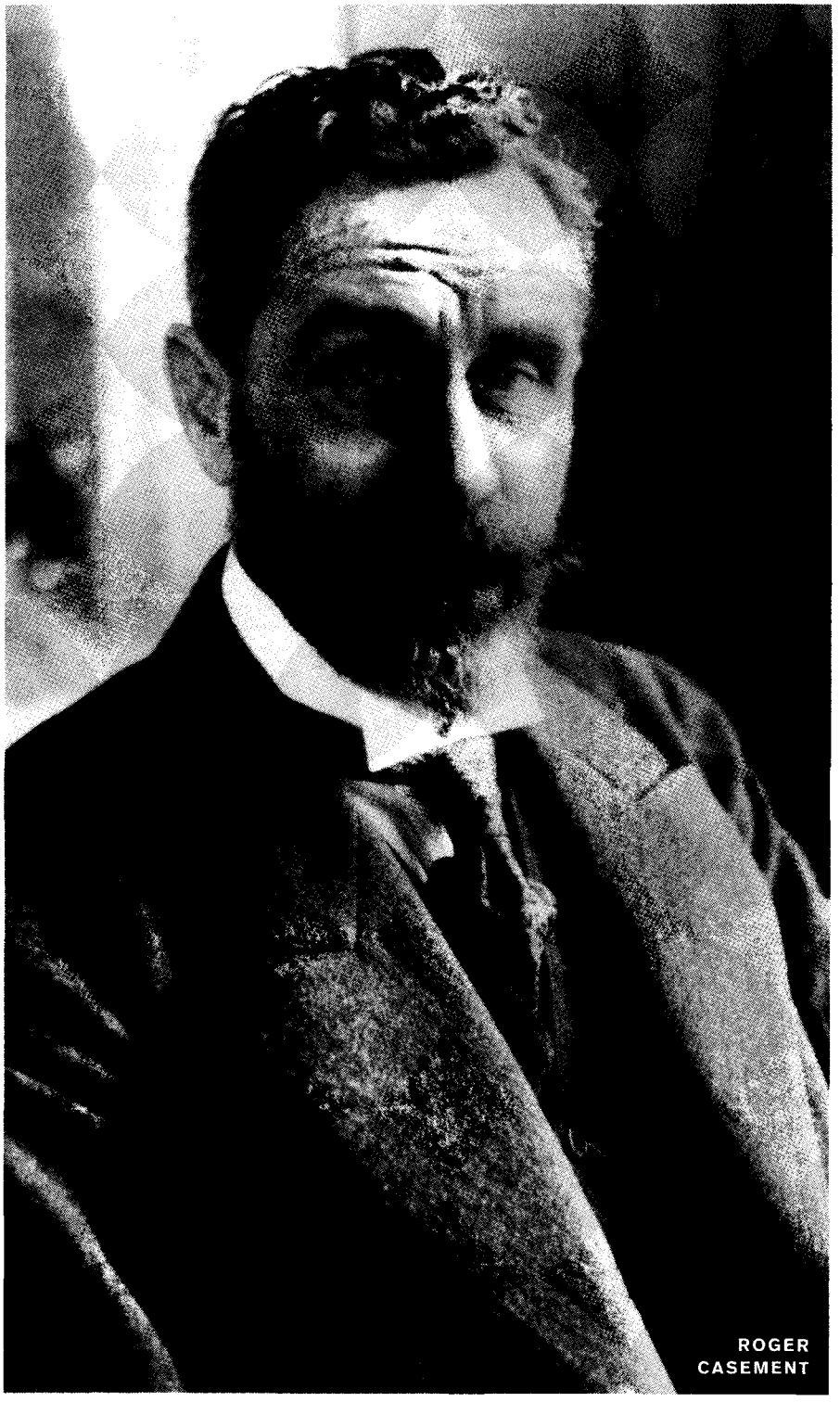
In the aftermath of the uprising, Casement was stripped of his knighthood, convicted of treason, and sentenced to death. What happened between Casement's sentencing and his hanging continues to cause heated disagreement among politicians and scholars. This much is clear: The British had a delicate balancing act to perform. They had already crushed the Irish rebels, but they did not want to alienate the United States, which was sympathetic to the Irish cause and still undecided about entering World War I. The British were determined to minimize Casement's appeal as a martyr.

While Casement was imprisoned in the Tower of London, Captain Reginald Hall, the director of naval intelligence, showed photographs of select pages of the alleged diaries to a number of English and American journalists and to opinion makers who were thought to favor a clemency petition for Casement. Meanwhile, rumors about the diaries' contents trickled out from several government departments, including the Foreign Office and the office of the home secretary; it was difficult to pinpoint the source of the leaks. The press referred to the diaries but did not quote them.

Thanks to prevailing attitudes toward homosexuality, the besmirching strategy was highly effective. The *News of the World* asserted that no one who saw the diaries "would ever mention Casement's name again without loathing and contempt." Otherwise-sympathetic public figures were discouraged from voicing their opposition to Casement's execution. For example, when the archbishop of Canterbury, Dr. Randall Davidson, heard of the diaries, instead of petitioning for clemency, he wrote a letter asking the home secretary to investigate the possibility that Casement suffered from a "mental disease, taking...a vicious form." The government prosecutor went so far as to offer the diaries to Casement's defense counsel, Seargeant Sullivan, to support an insanity plea. Sullivan rejected the proposition out of hand.

After Casement's death, the black diaries were kept under seal. But in 1922, the Fleet Street reporter Peter Singleton-Gates received a typescript of two of the diaries from an anonymous source. When he attempted to publish them in 1925, he was summoned to a personal interview with the British home secretary and threat-



ROGER CASEMENT

DID CASEMENT, A HUMANITARIAN AND IRISH REBEL LEADER, KEEP DIARIES RECORDING SEX WITH MEN? EVEN THE DIARIES' EDITORS CANNOT AGREE IF THEY ARE REAL.

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ened with prosecution under the Official Secrets Act. In 1959, Singleton-Gates at last defied the authorities by publishing the diaries in Paris with Maurice Girodias's Olympia Press, known for its risqué fare. Once two black diaries were available in print, the home secretary moved all four originals from their hiding place in Scotland Yard to the relatively accessible Public Record Office, where qualified scholars were finally permitted limited access to them. It was access that Casement's future biographers, including Roger Sawyer, would use to their advantage.

WHEN I SPEAK with Sawyer by telephone three months after the RIA conference, the retired preparatory school headmaster is at pains to present the nuances of his arguments with clarity. But he also seems eager to share his affection for Casement, whom he has studied for forty years.

Sawyer's passionate praise of Casement scans like iambic pentameter, with the emphasis falling on Casement's virtuous profile. "I know it sounds a bit corny, but he was incredibly brave. He sort of went into the jungle and did it himself. He galvanized the British Consular Service in the Edwardian era and made it an effective antislavery force. He was such a practical man and couldn't care less about his own safety."

Sawyer shares his hero's passion for the defense of human rights. He is a life member of the Anti-Slavery society, the oldest human rights organization in the world. The organization focuses on the enslavement of people worldwide and addresses issues like the "commercial sexual exploitation of children." Sawyer, the author of *Children Enslaved* (Routledge, 1988), has done work in Mauritania on the organization's behalf. He describes the society's methods as much like Casement's: It investigates abuses and "uses publicity as a weapon." He adds: "We hear rumors of some ghastly enslaving habit going on, and so we send somebody out and they make a report."

Sawyer first took an interest in Casement in the 1950s. An Anglican, Sawyer was vacationing with his Irish wife on Achill Island off Ireland's western seaboard. At the time, the black diaries were in the news because a member of Parliament from Ulster, H. Montgomery Hyde, known for his biography of Oscar Wilde, was agitating for their release from Scotland Yard. Sawyer recalls: "I was living in Ireland then and I rather accepted [that the diaries were forged], because everybody believed they were forged; it was part of the culture. I agreed that if they were put into the Public Record Office, I'd jolly well go and see what I made of them when I got back to London."

Sawyer did consult the diaries. (So did Hyde, who insisted the diaries were genuine in his 1960 book *Trial of Sir Roger Casement*.) In 1979, at the age of forty-eight, Sawyer completed a doctoral dissertation on Casement at the University of Southampton, which developed into his authoritative 1984 biography. Sawyer believed the diaries were indeed Casement's.

MITCHELL'S EPIPHANY CAME AS HE WORKED IN A LIBRARY: THE DIARIES "ONLY MAKE SENSE IF THEY HAVE BEEN FORGED."

CASEMENT was a skilled polemicist. During his lifetime, he published highly successful journalistic pieces of pro-Irish propaganda (he also made several feeble forays into poetry). His consular reports on the Congo (1904) and the Putumayo (1912) were masterfully crafted for maximum impact.

But until recently there was no definitive edition of either the black diaries or Casement's many uncontested journal writings, which are known as the "white diaries." The National Library of Ireland holds the two most substantial sets of white diaries: one written between 1914 and 1916, while Casement was in Germany; and the other in 1910 and 1911, during his investigation of abuses by the Peruvian Amazon Company.

The four black diaries are still held by Great Britain's Public Record Office. Each is a distinct size and make: a stubby Letts's Pocket Diary and Almanac for 1903; a foolscap-size, hardcover Dollard's Office Diary for 1910; a Letts's Popular Desk Diary for 1911; and a cash ledger with a number of textual entries for 1911. The 1903 diary corresponds to the period of Casement's investigations in the Congo, and the remaining three belong to his investigations in the Amazon. In his biography of Casement, Sawyer reports that three distinct handwriting styles appear in the diaries, corresponding to three distinct media: "a fine-nibbed scratchy pen, a broad-nibbed pen, and a pencil."

If the black diaries were not written by Casement, they were written by someone well informed about his movements, because they track his whereabouts closely. The entries record assignations with many individuals. The diarist cruises in public places, makes appointments, and then meets for sex. Often there are underlinings, which suggest, in Sawyer's view, that Casement was wont to return to his notes to reminisce over past blissful experiences. The diaries also contain some passages relevant to Casement's humanitarian work. The language is clipped, sometimes even coded. For example, "X" seems to stand for a sex act, probably anal intercourse. On March 2, 1910, the diarist writes: "Dark followed & Hard. Teatro Municipal. Breathed & quick *enormous push*. Loved *mightily*. *To Hilt Deep* X." He sometimes documents the size and shape of his partners' genitals. On May 26, 1910, he writes: "To Belfast, John McGonegal X 4/6. Huge & curved." He also records the amount of money spent in the pursuit of pleasure.

Belfast, London, and Buenos Aires are among the locations where the diarist enjoyed sex, often meeting the same person repeatedly. In fact, in 1910 and 1911, he seems to have been more prone to develop a social and emotional attachment to his partners than he had been in 1903, and the diaries for the later years are relatively more effusive. While on the Amazon, the diarist derives pleasure from the casual exhibitionism of the young men around him. For example, on October 29, 1910, he writes: "After dinner talked two Risigaros—muchachos—one a fine chap. He pulled stiff and fingered it laughing. Would have gone on & other too (keys

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on chain in left pocket) looking for *cigarettes*. Awfully exciting & stiff stiff work. Thought of Joao & Flores."

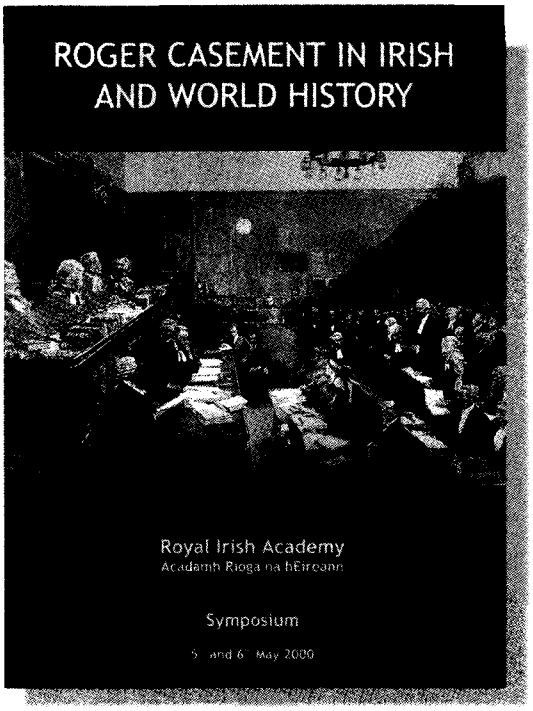
IN BRITAIN, documents deemed sensitive to national security have sometimes been kept from the public for as long as a hundred years, but in March 1994, as part of the Open Government Initiative, the black diaries were released to the public ahead of schedule. This was especially fortunate, as the partial transcription published by Singleton-Gates in 1959 was riddled with mistakes. To allow readers the opportunity to compare the black diaries with the white and draw their own conclusions, new and reliable transcriptions of all the diaries were clearly necessary.

In 1994, a young historian named Angus Mitchell saw the need for such an edition and approached Sawyer to propose a collaboration. Mitchell had earned a master's in modern history at Oriel College, Oxford, in 1983 and gone on to publish a couple of guidebooks on Spain in the early 1990s. When he met Sawyer, Mitchell had been living in Brazil for a year. His interest in Amazon history and environmental politics seemed an ideal complement to Sawyer's expertise on Casement. The two signed a contract with Pimlico Press in 1995.

At the outset, both Sawyer and Mitchell believed the black diaries were authentic. Both men spent time in the Public Record Office. Sawyer also corrected transcriptions in his home on the Isle of Wight, relying on a photocopy of the manuscript of the diaries, a typewritten copy made by Scotland Yard, and Singleton-Gates's flawed but still helpful edition.

But during a six-week trip to Dublin in 1995, Mitchell had an epiphany. "The eureka moment was when I finished reading through two boxes known as MS 13087 in the National Library of Ireland," he says. Those boxes contain one of Casement's white diaries, written on the Amazon in 1910, and other documents from the same period. Mitchell came to believe that these unquestionably authentic documents were factually inconsistent with the black diaries, which, he decided, must have been forged.

Mitchell's change of heart, two years into the project, surprised and baffled his collaborator. "When we were working together at first, we were going along together on the assumption that they were genuine," Sawyer recounts. "Then he went over to Ireland for a fortnight and came back saying, 'I think they were forged.' I said, 'Tell me why,' but he's never told me. He just says things like 'It's contrary to the historical record.'" According to Sawyer, Mitchell has never fully explained his reasoning.



For his part, Mitchell felt devastated and found it difficult to know what to do after his loss of faith. Friends advised him to abandon the Casement project, but he was reluctant to lose the investment of time and work. In addition to the edition he was working on with Sawyer, he had completed a historical account titled "The Putumayo Atrocities," on the assumption that the black diaries were genuine. "I'd written a whole book about human rights and Casement's voyage upriver as he had a lot of good sex," says Mitchell, "but when one actually starts stitching the documents together, they only make sense if they have been forged. Their ambiguity makes them brilliant forgeries."

Mitchell decided to part company with Sawyer in 1996. Independently, he contracted with the London-based Anaconda Press to produce an edition of Casement's 1910 white diary. In the preface to *The Amazon Journal of Roger Casement*, Mitchell asserts that according to his reading of the evidence, Casement was "the victim of a brilliant, though sinister, scheme hatched by British intelligence to prevent him attaining martyrdom." Mitchell leaves the intricacies of his argument to his footnotes, forty of which indicate apparent inconsistencies between the white and black diaries of 1910. For example, Mitchell points out that while in the Amazon, Casement suffered from an infection that nearly blinded him. Casement complains about it at least fifteen times in his white diary and correspondence, but it is mentioned only casually in the black diary. Because the infection obliged Casement to keep his writing to a minimum, Mitchell deems it unlikely he could have kept a second journal. He also points out that Casement refers to his "celibacy" in the white diary, a comment at odds with the sex-addicted persona of the black diary.

AT THE Royal Irish Academy conference in May, Mitchell positioned himself as the upholder of a Casement who, in his words, "had to be whiter than white." He thinks that homosexual activity would have jeopardized Casement's credibility as a witness to atrocities.

When we meet in Dublin, Mitchell and I sit for a while in a Paris-boulevard-style café across the road from the RIA. The city has been transformed in recent years, and as Mitchell and I talk, a waiter chases off an Eastern European child who has been begging from table to table. I note the changes in Dublin, and Mitchell remarks wryly, "It's like Mexico City."

Recently, thanks to the open-borders policy within the European Union, waves of Eastern European immigrants have been pushing the city beyond its socioeconomic comfort zone. A relatively large and sudden influx of Romanian refugees and a smaller immigration of Africans—one Dublin inner-city neighborhood has been renamed "Little Africa"—have strained the tolerance levels of a stereotypically easygoing people. Roger Casement, with his humanitarian concerns and his complex religious and political identity, could stand as an inspiring model for this increasingly diverse society.

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JEFFREY DUDGEON SPECULATES THAT CASEMENT'S DESIRE TO INTERVENE ON BEHALF OF INDIGENOUS MEN IN THE AMAZON MAY HAVE BEEN MOTIVATED BY SEXUAL INTEREST.

Mitchell explains that it was human rights issues in South America that fired his interest in Casement. In particular, he was attracted by Francisco "Chico" Mendes Filho, the Brazilian advocate for disenfranchised Amazon forest workers who helped organize a rubber tappers' union before he was murdered in 1988. Mitchell sees a historical continuity between contemporary struggles and Casement's investigations. And he's adamant in his belief that the consul's humanitarian side should not be upstaged by his sexuality.

It is through the lens of ethnography that Mitchell regards Casement's interest in the human body, which is evident even in the undisputed journals and the consular reports. He suggests that Casement wanted to give an accurate physical portrait of the Amerindians and Africans at a time when social Darwinism prevailed. The terminology in use in Casement's day "denigrated the stature of the Africans or Amerindians," Mitchell says. He characterizes Casement's own disposition toward the indigenous people as asexual: "When he sees a beautiful Indian, physically strong, as he does in the Boras [a tribe], he's not describing their physical beauty in a sexual way; he's giving the tribal people status."

Mitchell does concede that Casement uses a considerable amount of specifically sexual imagery in his Amazon white diary. But the imagery isn't a component of sexual adventure. On the contrary, it takes the form of a "horror of sexuality," according to Mitchell. "The abuse of sexuality becomes a way by which the tribal world is completely terrorized and destroyed or traumatized," he says. In their quest for rubber, Peruvian colonialists committed acts of sexual violence: "Casement hears appalling stories of rapes and murders and sexual cannibalism." Mitchell emphasizes that Casement's horror was typical of his era. "What Casement's constructing in terms of sexual horror [in the white diary] is utterly at odds with what is constructed sexually [in the black diaries]," he explains.

THE ORGANIZERS of the Dublin conference seemed sympathetic to Mitchell's view. Though the question of the diaries' authenticity was raised, the program omitted any direct reference to homosexuality. "I don't think going into the nitty-gritty of anyone's sexuality is going to help at all," says RIA employee Sara Whelan, one of the conference's organizers. "That's not what the focus of the conference was about."

Instead, the two-day conference focused on "Roger Casement in Irish and world history." Of course, the issue of sexuality was hard to avoid. During the Saturday-morning panels on the diaries, Sawyer argued for the authenticity of the diaries by disproving the negative: He methodically laid out what he saw as the four major arguments for forgery and then dismantled each of them.

1. *The sexual diary entries were written not by Casement but by a man named Armando Normand, whom Casement met while carrying out his investigations.* This hypothesis falls apart, Sawyer argued, because Normand happens to have been interested in women, not men. "The interpolation theory has always been somewhat absurd," Sawyer explained. "For all reasonable observers, this line of argument ceased the moment the original documents were produced."

2. *Many details in the black diaries are contradicted by passages elsewhere in the black diaries and by external evidence.* In fact, Sawyer argued, "the factual content also hung together satisfactorily." It would have been surprising if it hadn't, Sawyer noted; internal consistency would be a forger's top priority. Furthermore, the black diarist sometimes refers to entries in Casement's white diary, which can be corroborated. For example, on October 4, 1910, the black diarist writes: "Wrote out a lot of my diary since Friday."

3. *Before the circulation of the diaries, nobody ever remarked on Casement's homosexual propensities.* Again, the evidence suggests otherwise. "The historical record shows that he was a practicing homosexual given to reminiscing about events or to fantasizing about hoped-for events," Sawyer stated. Ernest Hambloch, who briefly served as Casement's vice-consul, "tells us his propensities were the talk of Rio," Sawyer said. Nowadays, even Mitchell agrees that Casement may well have been homosexual.

4. *The handwriting in the black diaries is not Casement's.* Sawyer cited a 1994 assessment of the diaries by the British Broadcasting Corporation. The television network commissioned Dr. David Baxendale, an independent handwriting expert, to analyze the diaries. He examined one erotic and one nonerotic entry and found both to be consistent with uncontested samples of Casement's handwriting.

In a query from the floor, one skeptical conference attendee asked how Casement could have had sex with men while working in close quarters with the commission investigating Amazon atrocities. A somewhat exasperated Sawyer explained later that nothing in the black diaries suggested he had: "If you look at the diaries, from the moment he joins the commission to the moment he leaves the commission, there was no monkey business—forgive the expression—there was no homosexual activity while he was with the commission." Sawyer further argued that the minute detail in the diaries would have been difficult if not impossible to forge. The 1911 cash ledger, for example, contains a record of "every penny Casement spent" that year. It puzzles Sawyer that some scholars seem to have a psychological need to prove the diaries were forged. After all, he says, "the British were absolute bastards" to use the diaries as they did, whether or not the diaries are authentic.

Those who believe the black diaries were forged often point to the fact that sex diaries attributed to Casement only exist for periods when the British Consular Service had accurate and copious information regarding his whereabouts and doings. But in his response to Sawyer at the RIA conference, Mitchell took an

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approach that was less empirical and more speculative. Although he presented a few instances of apparent inconsistency between the white and black diaries of 1910, he preferred, as he put it, to "deconstruct the Casement controversy and show how aspects of Casement's life and personality have been controlled through the use of the diaries." When I interview him later by phone, he adds, "If you want to believe them, of course you can believe them, and that's the brilliance of them."

Mitchell insists that the diaries are "littered with inaccuracies and inconsistencies." He adds: "It's not written in a language Casement uses anywhere else in his writings. Basically, it's a very typical piece of intelligence work, but [one] that requires a knowledge of World War I intelligence [to recognize]." Prior to the conference, Mitchell suggested to the London *Guardian* that navy intelligence head Reginald Hall might have forged them himself. Mitchell likens the British rumormongering about Casement to the panic caused in 1918 by a newspaper report of a list containing the names of forty-seven thousand people of questionable sexuality. Both propaganda campaigns, he says, aimed "to do essentially the same thing, to destroy a whole class as decadent and degenerate."

Mitchell thinks it's a mistake for gay people today to embrace what he considers dated, nasty British propaganda. In his opinion, Casement's diaries are unworthy of twentieth-century homosexual literature and present "the homosexual as an outsider, as a kind of loser." He recently told the *Irish Democrat* that the "Black Diaries controversy has succeeded in writing Casement out of history and obscuring his true stature behind a cloud of tedious sexual speculation." Nevertheless, Mitchell sees in the diaries a "dark beauty." He reckons that they were designed by their forger to "map a kind of Dantean journey of the homosexual."

Mitchell is still honing his arguments for forgery, and he's somewhat tight-lipped about them. Only about a third of those arguments are published, he says. He will reveal the rest when his current project—an edition of Casement's authenticated writings for 1911—is completed. Once those writings are compared with the two black diaries for 1911, he believes, the inconsistencies will be too glaring to ignore.

A LONGTIME believer in the diaries' authenticity, Roger Sawyer praises those researchers who have come to accept the black diaries as genuine despite their political allegiances. "Only in comparatively recent years have the barricades been crossed by brave men in search of the truth, however unsavory it might be," he wrote in his Casement biography.

At least one scholar has had no such barricades to overcome. As a gay Ulster Protestant, Jeffrey Dudgeon sees himself as a "perhaps unique" figure in the Casement debate. Dudgeon is completing a biography that will argue that Casement not only wrote the black diaries but also drank deep of the homoerotic pleasures of the Mediterranean. According to Dudgeon, Casement often took extended sick leaves and spent them in Italy: "He was in some ways very classically gay of that era."

When I speak by telephone with Dudgeon in his Belfast home, he has just

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come from a meeting with a delegation from PeaceWatch Ireland, a human rights group based in the United States. He tells me he disagrees with some members of the group about the importance of defending men arrested for public sex offenses. Dudgeon believes the gay community in the north of Ireland should be committed to the defense of men who are arrested.

Some participants in the Casement debate wonder if Dudgeon's assessment of the humanitarian isn't colored by modern-day sexual politics. "I suspect the diaries may have had quite an influence on him and the formation of his sexual consciousness, as they did with a lot of gay men [in Ireland]," Mitchell suggests.

In fact, Dudgeon's interest in Casement came long after his involvement with gay rights activism. But he does see Casement's sexuality as a factor in his effectiveness as a humanitarian—and he even finds Casement "sexually attractive." "I think if he hadn't been gay, he wouldn't have been as assiduous or as open-minded or as humanitarian," Dudgeon says. As Dudgeon paints him, Casement resembles a precursor of Jean Genet. Dudgeon even suggests that Casement's sexual interest in indigenous men may have motivated his desire to intervene on their behalf: "He fancied these guys...and he just didn't want to see them destroyed or ruined or put upon."

ENTERING THE debate from outside Anglo-Irish academic and political circles, Villanova's Lucy McDiarmid also spoke at the RIA event. She, too, believes Casement was homosexual (it was his poetry that first convinced her) and thinks it likely the diaries are genuine. "They were unarguably written by an active homosexual quite familiar with cruising grounds in Dublin, London, and Paris; with the economics, politics, and native peoples of the Amazon River area of Peru and Brazil; and with the geography of Ulster. Just how many people alive in 1910 fit that description?"

But the conference's closing speaker, Dr. Martin Mansergh, special adviser to the taoiseach, weighed in heavily on the side of forgery. Mansergh earned his doctorate for a study of prerevolutionary France, then joined the taoiseach's staff after a stint in the department of foreign affairs. At the RIA, he suggested that the burden of proof was on those who believed in the authenticity of the diaries, and he suggested that the apparently untainted Casement ought to be brought back into the nationalist pantheon: "I think it would be legitimate to co-opt him as a forerunner of Ireland's independent foreign policy tradition."

Sawyer was angry that no questions were allowed after Mansergh's speech, which he felt added little of substance to the debate. The conference seemed to end on a note of political expediency rather than free historical inquiry. When asked why the Irish government has apparently endorsed the view that the black diaries were forged, Sawyer speculates that it serves the current administration's political needs to excoriate the British for a historical injustice. "I'm sure that Ahern [the taoiseach] is a good man, but nevertheless he has been in cahoots with the British government to an embarrassing extent lately. Obviously, it would be rather nice to do a wonderfully nationalist act."

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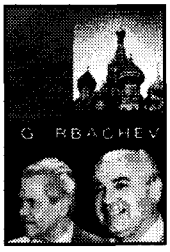

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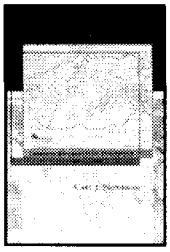

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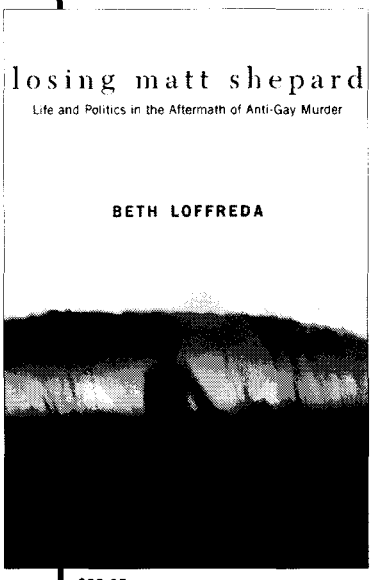
McDiarmid has a more sympathetic interpretation: "Now that Ireland is a player on the international scene, Casement can be invoked to show that Ireland has an international humanitarian tradition—to show that [former Irish president turned United Nations humanitarian officer] Mary Robinson is not an anomaly." Robinson, incidentally, was the first president in Irish history to welcome lesbian and gay people to her official residence, Áras an Uachtaráin.

In any case, the Irish government has taken an active interest in Casement. In 1999, the Irish Department of Foreign Affairs granted Angus Mitchell 30,000 Irish pounds (about U.S.$34,000) "for the completion of a seven-year study into the international academic debate about Roger Casement." In Sawyer's opinion, the grant "rather queers the pitch." Says Sawyer, "I think it's a bit dodgy of the Irish government to have offered him a large grant." He feels it would have been more appropriate for the government to have carried out an independent investigation: "They should have called in a neutral body, some Swede or Finn or someone, and said, you know, 'Come into this with no prior knowledge and thrash it out,' but not somebody who's totally committed."

But then, Casement has always been a peculiarly Anglo-Irish affair. Originally buried in Pentonville Prison in England, his remains were returned to Ireland in 1965 and he received a massive state funeral in Dublin. In a 1974 radio play by David Rudkin, an angry Casement complains from beyond the grave that his image, stripped of sexuality, has been exploited by both priests and politicians. He taunts a cardinal with his love of boys and rails against his interment in Dublin's Glasnevin cemetery rather than in his native county Antrim. The Irish government's grant to Mitchell could be seen as yet another attempt to erase Casement's sexual history. But among scholars, Mitchell remains rather isolated in his conviction that the black diaries are forged. Unless forensic analysis or dramatic new evidence changes the predominant view, it looks as if the Irish state will finally have to come to terms with a gay founding father.

Kieran Kennedy Jr. was born in Dublin, Ireland, and now lives in Allentown, Pennsylvania. He is completing a book-length manuscript on modernity and sexuality in Anglo-Irish relations.



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Special issue editor: Tom Sellar

The year 2000 marks the hundredth birthday of theater and opera composer Kurt Weill (1900–1950). *Theater* magazine celebrates this anniversary with "100 Years of Kurt Weill," a special issue devoted to the theatrical context of Weill's music.

This contemporary issue features two recently rediscovered and previously untranslated Weill librettos, *Royal Palace* and *Die Bürgschaft* (*The Pledge*). Critical essays and articles collected for this issue reflect on Weill's legacy and influence while exploring new perspectives on the artist, his work, and recent developments in Weill scholarship. These articles, combined with the previously unpublished works, make "100 Years of Kurt Weill" a considerable and unique contribution to the centenary commemoration of his birth.

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THE MYTH OF

A FORENSIC SCIENCE

A COURTROOM LOOKS DIFFERENT WHEN you are sitting on the witness stand. The imposing trappings of a medieval institution; the small crowd of silent onlookers; the elation that comes of debating issues one has long considered in isolation, tempered by the claustrophobia of the interrogation and the naked deployment of authority—it all seemed so reminiscent of...well, of my doctoral defense.

When I first chose to write my dissertation on how fingerprint examiners achieve credibility as expert witnesses, I never thought that I would end up an expert witness myself. But there I sat in the witness box, fidgeting nervously, swearing to tell the truth. Here was a role reversal worthy of these postmodern times, one that brought new meaning to the term "participant observation."

But for Byron Mitchell, a quiet, husky, bespectacled bear of a man in his mid-fifties, the proceedings were anything but academic. Mitchell was on trial for allegedly driving the getaway car in the 1991 robbery of an armored vehicle. No one had been hurt, but Mitchell, facing a conviction for armed robbery, was looking at spending most of the rest of his life in prison. The forensic evidence that linked him to the getaway car was the toughest sort to beat: two fingerprints lifted from the steering wheel and the gearshift. Mitchell's only hope was to undermine the credibility of the world's most widely trusted form of forensic proof. It was this endeavor that brought him to a federal courtroom in Philadelphia on a sweltering July morning, where he sat surrounded by four earnest public defenders, a determined prosecutor, some of the country's leading fingerprint examiners, three iconoclastic academics, and a patient but skeptical judge.

Most laypersons regard fingerprint identification as the epitome of science in the service of the law—indeed, as the model to which all other forensic sciences aspire. And yet this isn't quite so. Fingerprint identification puts the legal definition of science, recently articulated in the landmark 1993 Supreme Court decision *Daubert* v. *Merrell Dow*, to one of its most difficult tests. "Fingerprint evidence," Arizona State University law professor Michael Saks asserted in 1998, "may present courts applying *Daubert* with their most extreme dilemma.... A vote to admit fingerprints is a rejection of conventional

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FINGERPRINTS STANDS TRIAL

BY **SIMON COLE**

science as the criterion for admission. A vote for science is a vote to exclude fingerprint expert opinions."

The authority of fingerprint evidence rests on two contested assumptions. Although conventional wisdom since the nineteenth century has accepted the doctrine that no two fingerprints are alike, no one has really proven the proposition's validity. But if the question of the uniqueness of fingerprints seems pedantic, consider a more practical concern: How reliable is fingerprint evidence anyway? Can forensic technicians really match a fragmentary or smudged print taken from a crime scene to one and only one human fingertip, to the exclusion of all others in the world? At a pretrial hearing in the Mitchell case, this important question would receive its first airing in a U.S. courtroom.

FOR HUNDREDS of years, the uniqueness of fingerprint patterns has been so widely assumed that it never seemed necessary to prove it. As far back as the fourteenth century, the Persian historian Rashid ad-Din, reporting the use of fingerprints as signatures in China, declared that "experience shows that no two individuals have fingers precisely alike." At the beginning of the modern era of fingerprinting, John S. Billings, a U.S. Army physician, declared, "Just as each individual is in some respects peculiar and unique, so... even the minute ridges and furrows at the end of his forefingers differ from that of all other forefingers and is [*sic*] sufficient to identify."

The first person to fashion a statistical foundation for this assumption was the British gentleman scientist Sir Francis Galton. He calculated the probability that any two fingerprints would resemble each other in all particulars as one in sixty-four billion. Galton also noticed that the papillary ridges on the fingertip often end abruptly or split and rejoin one another. In his 1892 book *Finger Prints*, Galton labeled these points at which ridges end or split "minutiae"; they would later be known as "ridge characteristics," "points of similarity," and "Galton details." There are about thirty-five to fifty such points on a typical finger. By correlating the minutiae with one another, an expert could match two fingerprints.

Inspector Charles Collins of Scotland Yard put Galton's observations to work in the famous 1905 Deptford murder trial,

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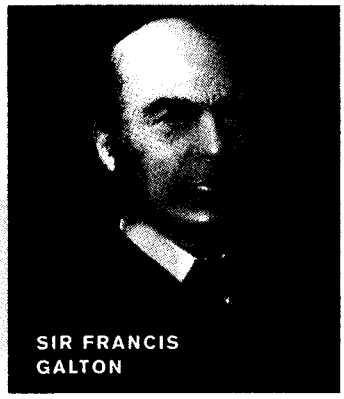
in which the brothers Albert and Alfred Stratton stood accused of murdering a shopkeeper and his wife in a London suburb. Collins compared Alfred Stratton's inked fingerprint with a bloody fingerprint found on a cash box and concluded that there were eleven matching "characteristics." The inspector's testimony convinced the jury but not the judge, who, bound by the jury's verdict, "almost apologetically" sentenced the Stratton brothers to hang. How secure was Collins's contention that he had matched the print to Alfred Stratton's finger, to the exclusion of all others? And how many matching minutiae did Collins require in order to reach his conclusion? No one really knew.

Oddly, the leading critic of Collins's methods was a Scottish physician named Henry Faulds—the very man who had first suggested to Galton (via Galton's cousin, Charles Darwin) that "greasy finger marks" might be used to link criminals to their crimes. In a pamphlet published just after the trial, Faulds complained that "the popular fiction, that no two fingers can be alike," was being treated as "a sober fact of the highest scientific certainty." And yet, he pointed out, "the only proof of it is seemingly the same 'fact' repeated in other words—that Scotland Yard by its system of classification has never been able to find two fingers alike." The absence of disproof, in other words, was being treated as proof. Moreover, based on his own experience studying fingerprints, Faulds claimed that "'repeat patterns' in single fingers are often found which come so near, the one to the other, that the least smudginess in the printing of them might easily veil important divergences with appalling results."

HENRY FAULDS



SIR FRANCIS GALTON

Why had Faulds gone from early champion of forensic fingerprinting to outspoken naysayer? To be sure, Scotland Yard's bigwigs had minimized the physician's contribution to the controversial new technique, and Faulds bridled at the slight. But he was also concerned that fingerprinting was slipping out of the hands of men like himself whom he considered "scientists" and into the hands of men like Collins whom he regarded as poorly educated clerks. "Dactyloscopy," as he called it, had once been a subject fit only for professionals like Galton and himself. But now, Faulds complained, it was becoming common "to treat the matter as one which can be dealt with by subordinate officials untrained in scientific observation."

Faulds was not entirely mistaken: During the first two decades of the twentieth century, control of fingerprinting shifted from scientists like Faulds and Galton to police identification clerks and detectives. In court, these clerks and detectives gave themselves the title FPE, but whether the *E* stood for "expert" or "examiner" was a subject of intense debate. And yet even as fingerprint identification fell less to scientists and more to cops, courts in the United States and around the world certified the practice as a "science."

Exactly what made fingerprint identification a science was not entirely clear. In 1928, in response to a defendant's demand for evidence "as to the state of this so-called science," the Vermont Supreme Court replied, "No such evidence was required. The subject is one of the things that does not have to be proved." Two years later, the Oklahoma Criminal Court of Appeals announced, "We take judicial knowledge that there are no two sets of finger prints exactly alike." In support of this opinion, the court cited a wide range of published literature—including legal treatises, court cases, criminalistics manuals, the *Encyclopaedia Britannica,* and Mark Twain's novella *Pudd'nhead Wilson*—all of which backed the assertion that there were no two prints alike. In 1941, the Texas Court of Criminal Appeals received a case in which a fingerprint was the only evidence against the defendant. The court undertook a thorough examination of the history of fingerprint identification and decided the time had come to reverse the burden of proof. "Instead of the state being called upon...to offer proof that no two finger prints are alike," the court wrote, "it may now be considered in order for those taking the opposite view to assume the burden of proving their position."

IT MAY BE true that all fingerprints are unique—in the sense that, as Gottfried Wilhelm Leibniz argued, all natural objects can be differentiated if examined in enough detail. But forensic fingerprint identification almost never deals in whole fingerprints. Rather, technicians use "latent" fingerprints—invisible impressions that they "develop" using a powder or a chemical developing agent. Latent prints are usually fragmentary, blurred, overlapping, and otherwise distorted. The challenge is to match the latent print to a pristine inked (or, these days, optically scanned) print taken under ideal conditions at the police station.

Today just about everybody, including attorneys, judges, and jurors, seems to accept that FPEs can reliably match latent prints to one and only one source finger. And yet some forensic scientists trained in fields like chemistry and toxicology remain skeptical. They charge that the FPEs avoid statistics, rely on mere hunches, and then proceed to couch their conclusions in terms of absolute certainty. The strongly held belief among FPEs that latent fingerprints can be matched to one person alone, wrote David Stoney in a 1997 legal practice manual, is "the product of probabilistic intuitions widely shared among fingerprint examiners, not of scientific research. There is no justification based on conventional science, no theoretical model, statistics, or an empirical validation process." Detailing the scrutiny visited on DNA typing, Stoney, director of the McCrone Research Institute, an Illinois nonprofit specializing in applied microscopy, declared, "Woe to fingerprint practice were such criteria applied!"

At the time, Stoney's words caught the eye of Robert Epstein, a public defender in Philadelphia. Epstein was helping Byron Mitchell's attorney prepare for Mitchell's second armed robbery trial. Epstein had already succeeded in getting Mitchell's first conviction overturned by the U.S. Court of Appeals for the Third Circuit in

FROM LEFT: JOHN BERRY; HULTON GETTY/ARCHIVE PHOTOS

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1998, when a crucial piece of evidence—an anonymous note that had been left on the windshield of the getaway car—had been deemed hearsay. Now the government was preparing to try Mitchell again. With the anonymous note excluded, the crucial evidence would be the two latent fingerprints lifted from the car.

After reading Stoney's article, Epstein realized that an opportunity had arrived: He would challenge the very admissibility of fingerprint identification as scientific evidence. He immediately contacted James Starrs, a professor of law and forensic science at George Washington University. Best known for his efforts to exhume historical figures like Jesse James and Meriwether Lewis to determine whether they had been murdered, Starrs is an enthusiastic proponent and practitioner of forensic science. At the same time, he is a critic of forensic techniques that he considers "garbage," like voice-stress analysis and handwriting identification. Fingerprint matching, he recently concluded, belongs in that category.

When Epstein put Starrs on his witness list, the prosecuting lawyers requested a pretrial hearing to examine Starrs's qualifications; they hoped to dismiss the testimony by casting doubt on the witness's scientific credibility. The defense countered by requesting another pretrial hearing—this one to address directly the question of whether fingerprints constituted reliable scientific evidence. It was the first time such a hearing had ever been held in an American court.

CAN EXPERTS MATCH A SMUDGED AND FRAGMENTARY PRINT TO ONE AND ONLY ONE HUMAN FINGERTIP?

JUST WHAT DOES the law consider science? It's a significant question, because the label the courts accord expert witness testimony can sway jurors from skepticism to sympathy. Until recently, the criterion for scientific evidence was "general acceptance" within a particular field. But this standard, critics pointed out, might apply as easily to astrology as to astronomy.

The Supreme Court reviewed the general-acceptance standard for the first time in the 1993 tort case, *Daubert* v. *Merrell Dow*, which examined allegations of birth defects caused by the morning-sickness drug Bendectin. Numerous parties, including the American Association for the Advancement of Science (AAAS) and the National Academy of Science, submitted friend-of-the-court briefs in order to help the Court reach a more sophisticated legal definition of scientific evidence. Justice Harry Blackmun's opinion acknowledged the difficulty, well known to philosophers of science, of devising hard-and-fast rules for distinguishing science from pseudoscience. Nonetheless, the Court agreed on four criteria: the testability, or "falsifiability," of the hypothesis; "submissions to the scrutiny of the scientific community" through mechanisms like peer review; the existence of an error rate and standards; and, again, general acceptance. Blackmun had borrowed the crucial notion of falsifiability from the philosopher of science Sir Karl Popper. Science, Popper argued, advances not by proving theories right but by proving them wrong, through a process of elimination. Scientific theories, according to Popper, can be "falsified," or proven wrong, by experiment. Unscientific theories—Marxist dialectical history and Freudian psychology were Popper's favorites—are formed in such a way that they cannot be falsified by data.

In adopting its new criteria, the Court cautioned that they were not strict requirements. They were to be taken only as guidelines for an assessment that, ultimately, was up to the judge's discretion. This flexible decision pleased nearly everyone. Plaintiff and defense lawyers alike read the decision as favorable to their interests, and the AAAS commended the Court for adopting the definition of science preferred by scientists.

But would latent fingerprint identification meet the new standards? If fingerprinting failed the *Daubert* test, would courts throw out fingerprint evidence, or would they throw out *Daubert*?

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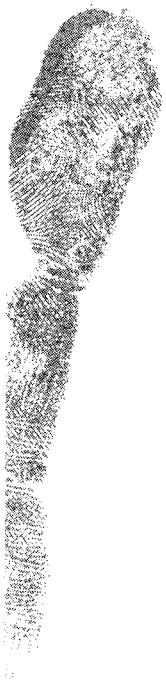
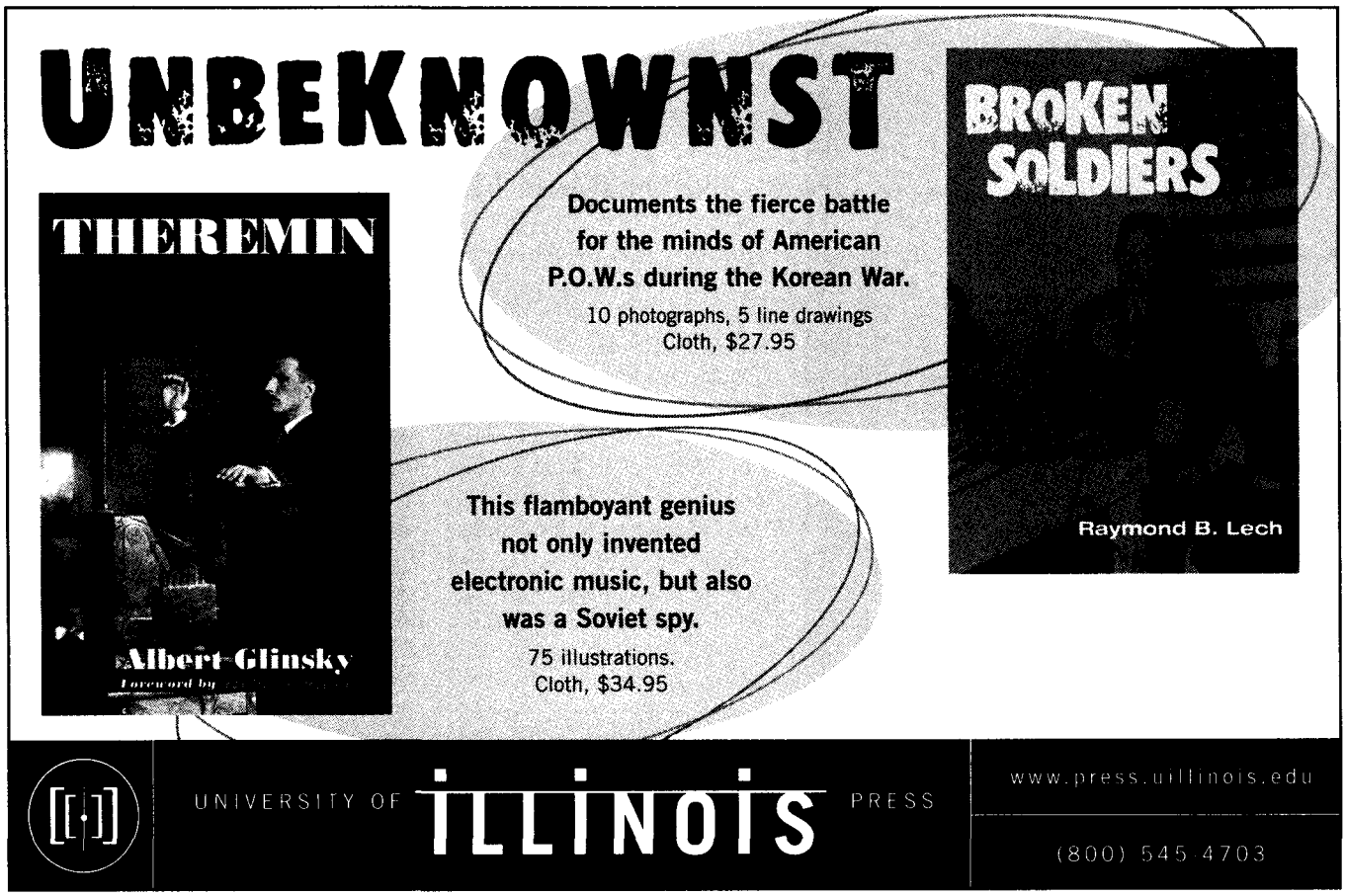
THE FBI SENT MITCHELL'S PRINTS AND THE ONES FROM THE GETAWAY CAR TO FIFTY EXAMINERS. THE RESULTS WERE INCONCLUSIVE.



FACED WITH a *Daubert* hearing, fingerprinting advocates couldn't exactly go out and compare prints for every finger in the world in order to prove the uniqueness hypothesis. Instead, the prosecution argued that the millions of fingerprint comparisons performed by thousands of law enforcement agencies worldwide in the course of case work constituted a kind of testing in itself. Furthermore, assistant U.S. attorney Paul Sarmousakis pointed to Galton's statistical model in his brief, contending that the sheer improbability of fingerprint duplication played to the prosecution's side. Sarmousakis further drew on a twins-based argument advanced by André Moenssens, a professor of law at the University of Missouri at Kansas City. General fingerprint patterns are thought to be genetically influenced, but the details of fingerprints are epigenetic—that is, determined by environmental forces, such as temperature and pressure in the womb. Therefore, although the fingerprints of identical twins are "more similar" than those of fraternal twins or unrelated individuals, they are still demonstrably different. If identical twins don't have identical fingerprints, Moenssens argued, we might assume that no one does.

In its brief, the defense attempted to dispense with all these arguments. The statistical models were flawed, the defense maintained, noting that none had been empirically tested. And Moenssens's twins argument did not address the problem of forensic identification—namely, whether fragments of two different fingerprints might accidentally match. As for the notion that case work itself constitutes empirical testing, the defense countered that such research fails to meet a basic requirement of falsifiability: There is no mechanism, short of extraordinary occurrences like postconviction exoneration, for determining that a latent fingerprint has been erroneously identified.

Perhaps anticipating such challenges, the Federal Bureau of Investigation made an eleventh-hour dash for empirical validation. In February 1999, Stephen Meagher, a supervisory fingerprint specialist at the FBI, sent a letter to all fifty state crime laboratories that began, "The FBI needs your immediate help!" Enclosed were copies of the two latent prints from the getaway car and an inked fingerprint card containing a full set of Mitchell's ten fingerprints. The respondents were to examine the latents in order to determine whether they matched any of the prints on the card. The idea, presumably, was to show that latent print identification was objective by demonstrating that different practitioners would agree on whether or not the prints matched.



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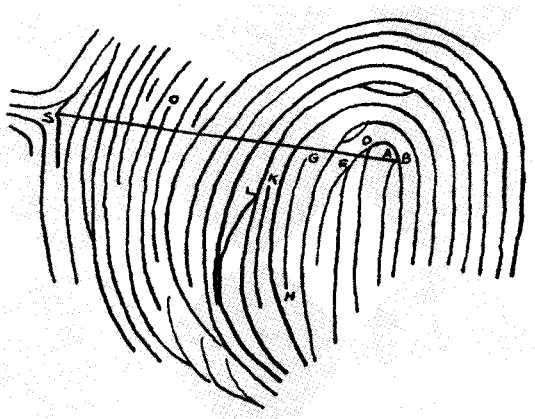
But rather than clarifying matters, Meagher's survey added yet another wrinkle to the case. Seven laboratories failed to match one of the latent prints with Mitchell's inked prints, and five failed to match the second latent print. Suddenly, Epstein had a number of qualified FPEs who said that the latents from the getaway car could not be positively identified as Mitchell's prints. The prosecution had a problem.

Meagher quickly sent a follow-up package to all the laboratories that failed to identify one or both of the prints. The package contained better photographs and enlargements of both the original lifted prints and the prints from Mitchell's fingerprint card that were supposed to match them. The enlargements arrived in plastic sleeves on which Meagher had marked corresponding ridge characteristics with red dots. Meagher's letter asked the state examiners to "test your prior conclusions against these enlarged photographs with the marked characteristics" and to return the results as soon as possible. Almost all the respondents changed their minds and declared that both prints matched.



RIDGE COUNT WITH CHARACTERISTICS

From the defense's point of view, the FBI's follow-up "survey" was a blatant attempt to intimidate state examiners into changing their opinions. According to ideal protocols, FPEs should not use enlargements to examine latent prints. Moreover, marking up a print for another examiner is considered highly improper. At the hearing, however, Meagher compared the follow-up to a training exercise: "If an individual fails to make an identification that we believe they should have been able to, we would take that information back to that individual, show them the characteristics which they should take into consideration, ask them to reassess their position and, you know, use the information that's now presented to them to try to come up with the same conclusion. That is, that the two prints were identical." The study was not a scientific experiment, Meagher insisted; it was "just a survey."

In any event, guarding itself against the possibility that the survey would fail to sway the court, the FBI used a computer model to clinch its case. This idea had been knocking around law enforcement agencies ever since the mid-1980s, when Automated Fingerprint Identification Systems (AFIS) began to emerge. With millions of fingerprints optically scanned into fingerprint databases, why not have the computer search for matches between them? Such an endeavor might answer

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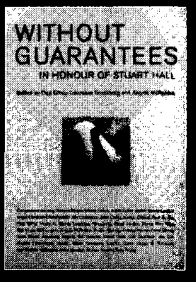

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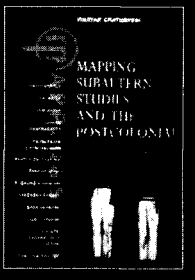

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Faulds's old criticism—that police agencies had never looked for two very similar fingerprint fragments from different people.

In conjunction with Lockheed Martin, the aerospace company that built its AFIS, the FBI performed what it called a "50K vs. 50K Fingerprint Comparison Test." The test involved running each of fifty thousand single fingerprints through the AFIS against the whole set, and generating a similarity score (a "Z score") for each comparison. The result was that no two different fingerprints generated a Z score anywhere near the Z score for a fingerprint compared with itself.

Could the FBI rest its case? Not so fast. As Epstein revealed in his cross-examination of Donald Zizek, a Lockheed engineer, the test had generated the benchmark Z scores for matches by comparing each fingerprint with itself. But forensic fingerprint identification is supposed to compare two *different impressions* from the same finger. As Stoney noted, "No two things, no two representations of this person's signature, no two representations of their fingerprint will be exactly alike." The test, therefore, ought to have used two different impressions from the same finger to establish a baseline score for a match. "It is really shocking," Stoney testified, "to see something presented...that doesn't have that basic element of forensic examination in part of the study."

THE GOVERNMENT'S star witness at the *Daubert* hearing was David Ashbaugh, a tall, crisp, and genial Canadian Mountie. An indefatigable writer and researcher, Ashbaugh has more or less single-handedly tried to articulate a scientific foundation for latent fingerprint identification that meets modern standards of "science." In doing so, his body of work has also called some cherished principles of fingerprint identification into question.

For most of the twentieth century, FPEs used the minutiae method Galton devised in the 1890s. What methodological debates there were centered on how many minutiae, or "points of similarity," ensured a positive identification. Some police departments thought twelve, some thought ten, some were comfortable with eight. By the 1970s, however, many FPEs, aware of the growing sophistication of other forensic sciences, had realized that the various numerical standards were not based on empirical studies but were simply cautionary guidelines meant to preclude misidentifications. In 1973, the professional organization of FPEs, the International Association for Identification (IAI), declared that there was "no scientific basis" for adhering to any minimum standard. Instead, the IAI argued that the criteria for a match should be the examiner's expert opinion.

Since the 1980s, Ashbaugh has expanded upon the IAI's position, sternly opposing the use of point matching in fingerprint identification. Ashbaugh advocates a holistic "quantitative-qualitative evaluation process" that encompasses all

"*Paradigm* was a perfectly good word, until I messed it up."
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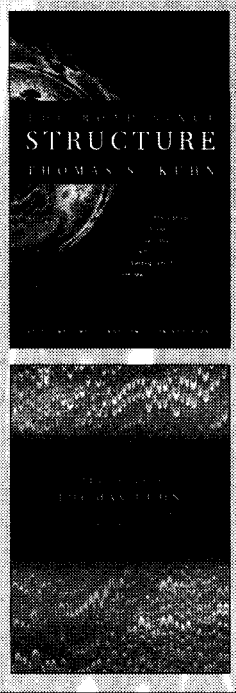

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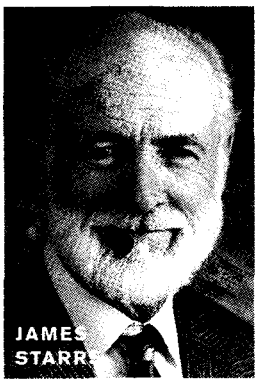
available information in latent prints, including points. But what constitutes a match in Ashbaugh's system is not clear. "That final decision," Ashbaugh testified at the *Daubert* hearing, "is a subjective decision. It's based on your knowledge and experience and your ability."

Not only Ashbaugh but all the government witnesses claimed that point counting was an antiquated method, rendered obsolete by Ashbaugh's work. Indeed, some refused even to use the term "points." At Mitchell's original trial, however, FBI special agent Duane Johnson had shown the jury charts with matching points. And at Mitchell's second trial (held in February 2000, after the hearing), the defense called numerous state FPEs, who emphasized point counts and professed no knowledge of either Ashbaugh or his method. But according to Ashbaugh, even FPEs who think they are counting points are unconsciously doing quantitative-qualitative evaluation. Indeed, the FBI's Meagher insisted that courtroom charts displaying matching points are "just a simplistic way of explaining the identification process to the jury." The point minimums, meanwhile, were "strictly" for "quality assurance,...not for individualization."



In championing Ashbaugh's holistic evaluative technique, the prosecution claimed that fingerprint identification was essentially an error-free science. True, erroneous fingerprint identifications had been exposed in the past, and proficiency tests reveal high rates of error by FPEs. But at the hearing, Bruce Budowle, a senior scientist at the FBI laboratory in Quantico, Virginia, who holds a doctorate in genetics and is one of the country's leading authorities on DNA typing, maintained that because FPEs improve and refine their methods in response to exposed gaffes, using these errors "to calculate an error rate is meaningless and misrepresents the state of the art."

Meagher took these claims a step further, contending that the true error rate for fingerprint identification was zero. He did this by distinguishing "methodological error" from "practitioner error," concluding that even if practitioners occasionally made mistakes or operated under less-than-optimal conditions, the methodology of fingerprint identification was beyond reproach. "If the scientific method is followed," Meagher testified, "the error in the analysis and comparative process will be zero."

AFTER THE prosecution rested, the defense countered with Starrs. Although recovering from recent surgery, Starrs, leaning on a wooden cane, clearly relished taking the witness stand. He called himself an "eccentric" and an "iconoclast"; he made frequent reference to his eight children and to his wife, who he said possessed "a fiery Irish temper"; he engaged in repartee with both the judge and the attorneys; and when he suspected the prosecutor of trying to corner him, he said so, declaring, "I will not

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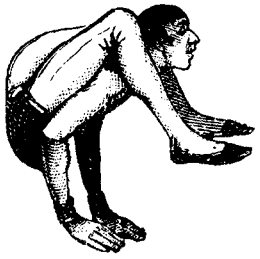

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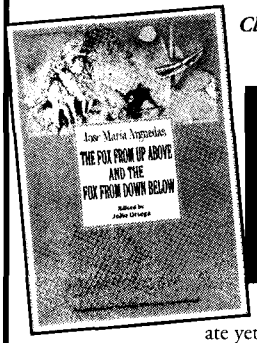

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be trapped." Starrs attacked fingerprinting advocates for "the claim of absolute certainty...the failure to carry out controlled empirical data searching experimentation, a failure to recognize the value of considerations of the error rate." Error, after all, is a necessary accompaniment to measurement and experiment: No academic scientist would claim, or even want to claim, to have a zero error rate. Nor would he wish to calculate an error rate based on ideal conditions not present in actual practice. Starrs further deplored "the lack of objectivity and uniformity and systemization with respect to standards, if any, of the fingerprint analysis." He concluded: "It is a miasma. It just has absolutely no basis at all in solid science."

Starrs was followed by David Stoney and myself. Stoney argued that case work did not constitute testing. "Remember," he testified, "to be science it has to be capable of proving that I'm wrong." But case work provides no mechanism for exposing errors. Bringing to bear my work on the history of fingerprint identification as a forensic tool, I tried to explain how and why the practice's premises had never been tested. But called to the stand to challenge a discipline's claims to expertise, I, predictably, ended up having my own expertise challenged. In the end, Judge J. Curtis Joyner ruled that only fingerprint examiners were qualified to critique fingerprint identification and that neither Starrs nor I would be permitted to testify in front of the jury. Stoney might be allowed, but only as a fingerprint examiner with an opinion on the case prints themselves.

By persuading the judge to bar expert testimony against the scientific standing of fingerprinting, the prosecution effectively won the *Daubert* hearing. The barring of such testimony, of course, is reminiscent of an argument that has plagued science studies since its inception. Practitioners of the disciplines we study object to our work on the grounds that we don't have enough training in their disciplines to criticize their knowledge claims. But can vigorous skepticism, which courts and philosophers agree should characterize a science, be achieved by a discipline's practitioners alone?

Last summer's *Daubert* hearing failed to strike down fingerprint evidence, but it raised important issues that continue to spark fierce debate. In the wake of the Mitchell case, somewhere between fifteen and twenty *Daubert* challenges to fingerprint evidence have been filed around the country. At the same time, conflict continues to divide the fingerprinting community, where Ashbaugh's opponents still defend point standards.

As for Byron Mitchell, at his second trial, the defense subpoenaed all the state examiners who failed to match the latent prints during Meagher's first survey. But the defense's attempt to insinuate that the FBI had intimidated these examiners into changing their opinions apparently did not sway the jury: Mitchell was convicted on all counts. The jury deliberated a little more than two hours. Epstein, however, plans to appeal his case yet again. And the deliberation over the scientific basis of fingerprint evidence, no doubt, has just begun.

Simon Cole is the author of *Suspect Identities: A History of Fingerprinting and Criminal Identification,* forthcoming this spring from Harvard University Press.

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REQUEST FOR PROPOSALS

The Council is pleased to announce the 2001 competition of the International Dissertation Field Research Fellowship Program, which provides support for humanists and social scientists to conduct dissertation field research in all areas and regions of the world. Up to fifty fellowships will be awarded in 2001. The program is administered by the Social Science Research Council in partnership with the American Council of Learned Societies. Funds are provided by the Andrew W. Mellon Foundation.

The fellowships enable doctoral candidates to use their knowledge of distinctive areas, cultures, languages, economies, polities, and historical experiences, in combination with their disciplinary training, to address issues that transcend their disciplines or area specializations.

Fellows participate in multi-disciplinary workshops upon completion of their field research. Workshops highlight fellows' research agendas and address themes that resonate across cultures and regions. They are intended to facilitate networks and cross-disciplinary exchanges, and to help fellows engage in issues beyond their doctoral research.

ELIGIBILITY AND TERMS

The program is open to full-time graduate students in the humanities and social sciences—regardless of citizenship—enrolled in doctoral programs in the United States. The program invites proposals for field research on all areas or regions of the world, as well as for research that is comparative, cross-regional, and/or cross-cultural. Proposals that identify the U.S. as a case for comparative study are welcome; however, proposals that require no field research outside the United States are not eligible. Proposals requesting support for a second year of field research will be funded only under exceptional circumstances.

Applicants must have completed all Ph.D. requirements except the field research component by the time the fellowship begins or by December 2001, whichever comes first.

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Applicants are expected to write in clear, intelligible prose for a selection committee that is multi-disciplinary and cross-regional. Proposals should display a thorough knowledge of the major concepts and methods relevant to the research, both in the applicant's discipline or subfield and in other fields where appropriate. The proposed research will be assessed in terms of the probability that it can inform debates that go beyond the specific topic and place chosen for study. Applications should exhibit a grounding in the methods and theories of a particular discipline or subdiscipline, but also must of demonstrable cross-disciplinary interest.

Applicants should specify why an extended period of field-based research is critical to the successful completion of the proposed doctoral dissertation. The research design of proposals should be realistic in scope, clearly formulated, and responsive to theoretical and methodological concerns.

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ANNOUNCEMENT OF AWARDS

April 30, 2001

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Standard fellowships will rarely exceed $18,000 in field research support. In exceptional circumstances, the candidate may propose less than nine months of fieldwork, but no award will be given for less than six months of fieldwork. The fellowship must be held for a single continuous period within the eighteen months between July 2001-December 2002.

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APPROACHES TO TEACHING STOWE'S

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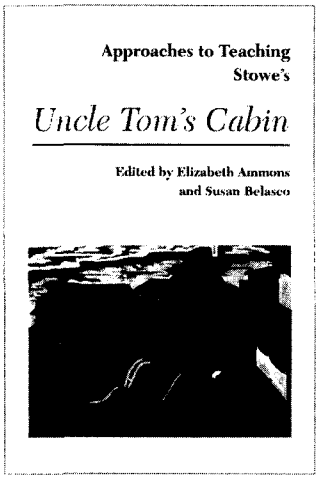

JUNE 2000

325 pp.
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ALTHOUGH RARELY found on college syllabi just two decades ago, *Uncle Tom's Cabin* is (according to an MLA survey) one of the most frequently named additions to nineteenth-century American literature courses. The inclusion of this political, sentimental, and incredibly popular novel introduces a host of issues to the classroom: the novel's place in the canon of women's literature, the historical importance of its commercial success, the status of Stowe's work as "good" literature, and—perhaps the greatest challenge to teachers—the topic of race.

This volume, like others in the MLA series Approaches to Teaching World Literature, is divided into two parts. The first part, "Materials," reviews available editions of *Uncle Tom's Cabin*, biographical works, historical materials, works of criticism, and audiovisual resources. The seventeen essays in the second part, "Approaches," suggest teaching strategies that spotlight the novel's literary and historical context, recent debate and controversy, and current theoretical and critical methodologies. Because the issue of race tends to dominate any attempt to teach or discuss the novel, a number of essays address the racism that pervades Stowe's best-known work.

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continued from page 7

I did. I called Flora Pavlovna Litvinova and asked her whether her statement referred to a conversation with Shostakovich before work on *Testimony* had begun (1971) or after, because of her earlier statement that her last "meeting" with the composer was in 1971. Her answer: "I ran into Shostakovich here and there until his death. The conversation in question could have taken place in 1972, or 1973, or 1974." I then asked her, "Do you think Shostakovich was referring to *Testimony* or some other work he did with Volkov?" Her answer: "I understood it to be referring to *Testimony*."

So here. The gun still smokes.

DMITRY FEOFANOV
Naperville, Illinois

Paul Mitchinson replies: Although Dmitry Feofanov has invented a quotation he attributes to me, and has misquoted Flora Litvinova's published account, it's still possible he has accurately quoted what she told him over the phone.

Litvinova's account, which she wrote in the late 1980s, was published in 1996. She did not write that her last "meeting" with Shostakovich was in 1971; she wrote that her last "conversation" (*razgovor*) with the composer took place then. Her memory was quite exact: "My last conversation with Dmitri Dmitrievich took place at the House of Creativity in Ruza in 1970 or 1971. He had returned from having treatment at Dr. Ilizarov's clinic [in Kurgan]." These details can be independently confirmed. Several months later, Volkov's first meeting with Shostakovich for *Testimony* allegedly took place.

If, under Feofanov's cross-examination more than ten years after her initial statement, Flora Pavlovna now remembers additional "conversations" taking place between 1972 and 1974, then what we have is no longer a smoking gun but rather, like almost everything having to do with *Testimony*, conflicting and ambiguous evidence.

HOME NEWS

As many of our readers are aware, *Lingua Franca* published an article in 1996 titled "A Physicist Experiments With Cultural Studies." In that article, New York University physicist Alan Sokal revealed that an essay he had published in *Social Text* was in fact a parody of the poor thinking and opaque writing rampant in science studies and postmodernism. Now, in collaboration with the University of Nebraska Press, *Lingua Franca* has published a book titled *The Sokal Hoax: The Sham That Shook the Academy*. The book offers the definitive guide to the debate that ensued over Sokal's prank. It includes the original text of Sokal's hoax article, responses from the editors of *Social Text* and other academics, a dossier of U.S. and foreign media reports on the case, and longer essays that explore what it all means.

Lingua Franca is also updating its *Real Guide to Grad School*, which was originally published in 1997. The guide now includes chapters on newly emerging fields such as ethnic studies, museum studies, performance studies, and cognitive science, as well as on the contentious place of teaching in graduate student life and how to make sense of all those university rankings. Details are available at www.linguafranca.com/realguide.html.

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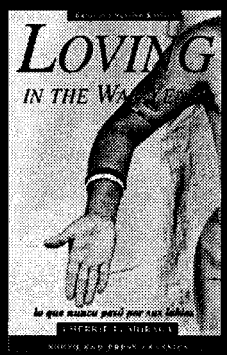

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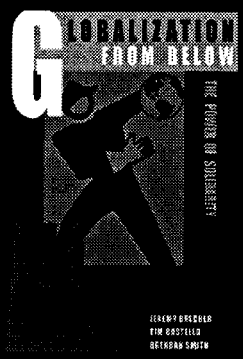

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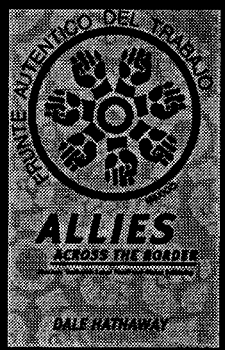

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New Haven, CT: December 8–9, 2000

ORGANIZATION OF AMERICAN HISTORIANS

The Organization of American Historians announces its 94th Annual Meeting to be held April 26–29, 2001 at the Westin Bonaventure Hotel in Los Angeles, California. The meeting's location in the cultural mosaic of Los Angeles and OAH's efforts to reach a wider public helped shape our theme, "Connections: Rethinking Our Audiences." Over 180 sessions, panels, workshops, and round-table conversations offer the best scholarship in American history and culture. Featured speakers include NEH chair William Ferris, documentary filmmaker Ric Burns, and political commentators Arianna Huffington and Susan Estrich. The world's largest professional association of American history educators and researchers, OAH publishes the *Journal of American History*. Web: www.oah.org; E-mail: <oah@oah.org>; T: (812) 855-7311.

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THE LOOKING-GLASS WAR

WHY DON'T WE SEE OURSELVES UPSIDE DOWN IN THE MIRROR?

THE OTHER DAY AS I WAS wandering down an old tenement block on New York's Lower East Side, I chanced upon an odd little shop. It sold only one item: mirrors that do not reverse left and right, or True Mirrors, as the store calls them. There was one in the shopwindow. Looking at my reflection in it, I was appalled by how crooked my facial features seemed, how lopsided my smile was, how ridiculous my hair looked parted on the wrong side of my head. Then I realized that the image I was confronting was the real me, the one the world sees. The image of myself I am used to, the one I see when I look into an ordinary mirror, is actually that of an incongruous counterpart whose left and right are the reverse of mine.

There is nothing very strange about the fact that ordinary mirrors reverse left and right, is there? "Left" and "right" are labels for the two horizontal directions parallel to the mirror. The two vertical directions parallel to the mirror are "up" and "down." But the optics and geometry of reflection are precisely the same for all dimensions parallel to the mirror. So why does a mirror treat the horizontal and vertical axes differently? Why does it reverse left and right but not up and down?

This question might seem foolish at first. "When I wave my right hand, my mirror counterpart waves his left hand," you say. "When I wiggle my head, I should scarcely expect my counterpart to wiggle his feet." True enough, but you might plausibly expect your counterpart to appear upside down, with his feet directly opposite your head—just as his left hand is directly opposite your right hand.

Foolish or not, the issue has been vexing philosophers for at least half a century now. As far as I can tell, it first arose in the early 1950s, as a sort of sidebar to discussions of Immanuel Kant's theory of spatial relations. In his 1964 book *The Ambidextrous Universe,* the science popularizer Martin Gardner stirred things up by arguing that the puzzle has a false premise. A mirror does not really reverse left and right at all, he claimed; instead, it reverses front and back along an axis perpendicular to the mirror. In his view, we merely "find it convenient" to call our image left/right reversed because we happen to be bilaterally symmetric. In 1970, the philosopher Jonathan Bennett published an article endorsing Gardner's supposed resolution of what by now had become the "mildly famous mirror problem."

But the sense of closure was premature. In 1974, the MIT philosopher Ned Block wrote a long, diagram-filled piece for the *Journal of Philosophy* in which he contended that the question "Why does a mirror reverse right/left but not up/down?" has at least four different interpretations. Block claimed that the four interpretations had been clumsily conflated by Gardner and Bennett; he also insisted that in two of the four, a mirror really does reverse left and right. Three years later, in an equally lengthy article that appeared in the *Philosophical Review,* an English philosopher named Don Locke declared that Block was only "half right." Mirrors actually reverse left and right in *every* relevant sense, he argued.

Reading these papers, and others that have appeared since, one comes to feel that the mirror problem defies philosophical reflection. People can't seem to agree on the most basic facts. For example: Stand sideways to a mirror, shoulder to shoulder with your image. Your left/right axis is now perpendicular to the mirror's surface. Gardner and Bennett say that in this case and this case alone, a mirror really does reverse left and right. Block and Locke say that in this case and this case alone, left and right are the *same* direction for you and your mirror image. (Having just dashed into the dressing room to try this, I *think* I'm in the Block-Locke camp. My right arm and my mirror counterpart's right arm both pointed east; on the other hand, he was wearing his watch on his right wrist, whereas I wear mine on my left.)

The key to the mirror puzzle would seem to lie in some subtle disanalogy between left/right and up/down. Both of these pairs of directions are relative to the orientation of the body (unlike, say, east/west and skyward/earthward). But as any child will attest, left/right is much harder to master than up/down. The human body displays no gross asymmetries between its two sides. (There is, of course, the heart, but that is hidden.) So "left" and "right" have to be defined in terms of "front" and "head": Your left hand is the one that is to the west when you stand on the ground and face north. This would remain true even if a surgeon cut off your two hands and sewed them onto the opposite arms.

Left/right is thus logically parasitic on front/back, whereas up/down is not. And a mirror, everyone agrees, reverses front and back. That must be why it also reverses left and right—if indeed it does, which to this day remains unclear.

Fatigued by the debate? Then visit the little shop I discovered (www.truemirror.com) and get yourself a True Mirror. But don't try shaving in front of the thing—your face will be a bloody mess.

JIM HOLT

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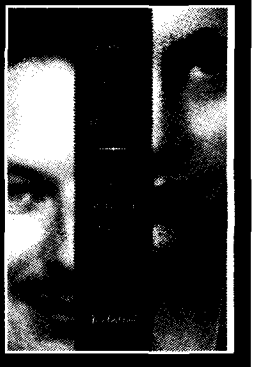

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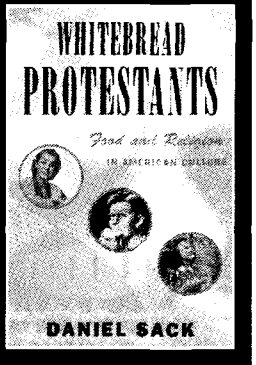

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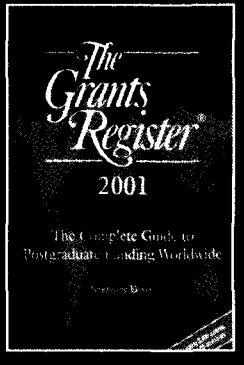

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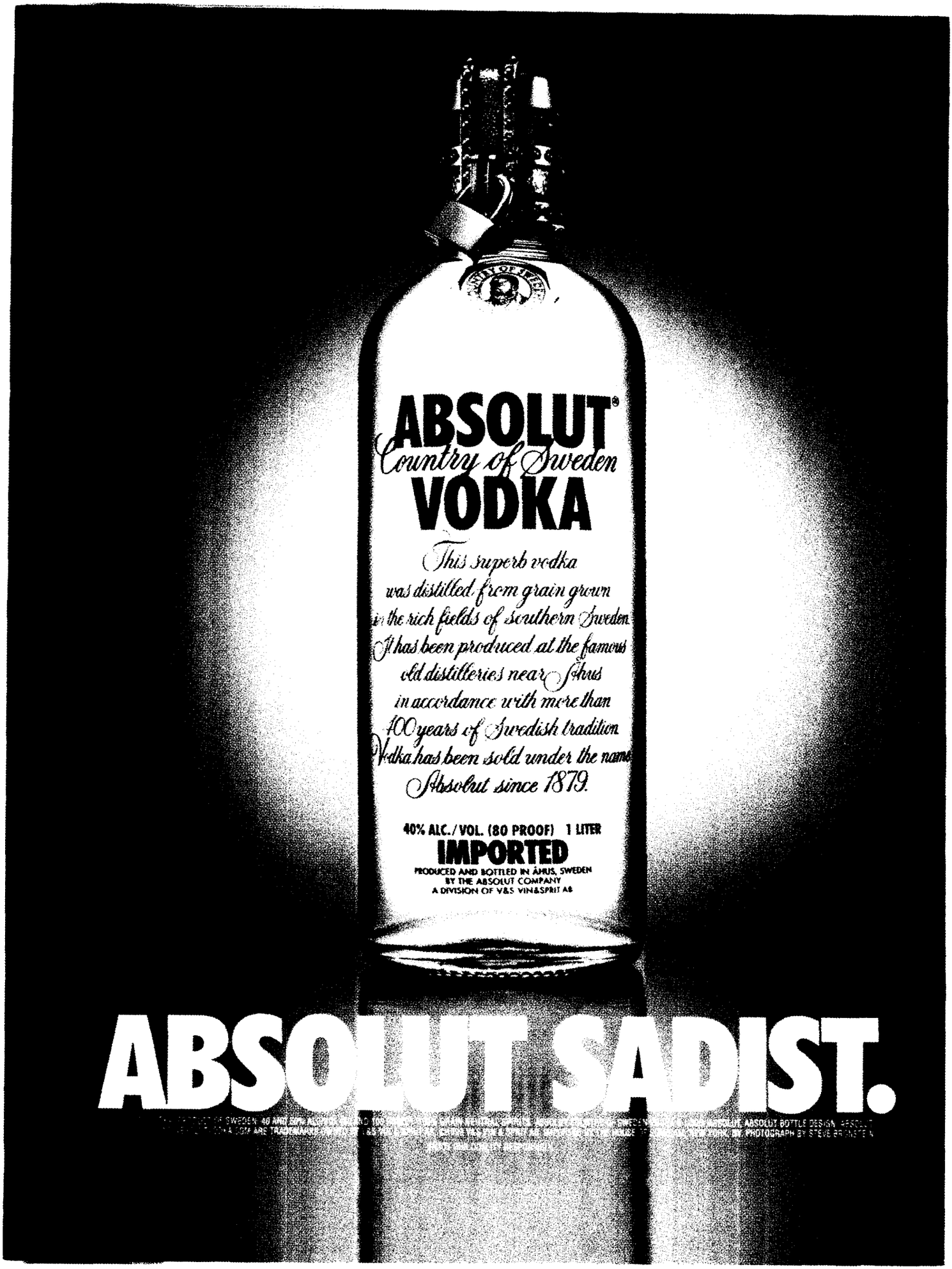
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